Science Progress.

No. 6. AUGUST, 1894. Vol. I.

RECENT WORK ON THE MORPHOLOGY OF TISSUES IN THE HIGHER PLANTS.

THE classification of tissues proposed by Sachs in his *Text-book of Botany* has been until quite recently the arrangement most familiar to English students. Sachs' three systems of tissue, the Dermal, the Fascicular, and the Fundamental or Ground Tissue, have, however, a merely descriptive value, and cannot claim to rest on any consistent basis, whether physiological or morphological. The first two systems are indeed broadly characterised by function, but the third is obviously made up of the most heterogeneous constituents, performing every kind of physiological work.

That the arrangement was never intended to be a strictly morphological one is sufficiently shown by the fact that the Dermal Tissue includes not only the epidermis, but the periderm, a secondary formation which may arise from any tissue whatsoever, and is only in exceptional cases of common origin with the epidermis.

De Bary's great work on the *Comparative Anatomy of the Phanerogams and Ferns*, invaluable for the immense mass of well-ordered detail which it contains, and for its critical treatment of certain developmental questions, did little for the general morphology of tissues. The "Forms of Tissue," to which the first part of the book is devoted, are classified by purely histological characters, *i.e.*, according to the mature structure of their elements. The second part

treats of the arrangement of tissues, but except for the two main headings—primary arrangement and secondary changes—there is little attempt at a natural grouping. De Bary's book, while unrivalled as a storehouse of facts, perhaps tends to leave on the mind of the reader the impression that anatomy is after all nothing but an accumulation of detail, and that a morphology of the tissues, based on homology, and comparable to that of external organs, does not exist. That this should be so is no reproach to the author, who saw clearly that a scientific morphology must be founded on development, and thought that the developmental data were too conflicting to form a satisfactory basis for classification (1, p. 22). This difficulty still exists; even now the attempt to found an internal morphology of plants has only met with very partial success. The problem is of great complexity and will probably never be completely solved. Still the attempt has been made, and has given new life to the study of anatomy.

It is to the French botanist Van Tieghem, more than to any other individual, that the recent progress of internal morphology is due. This statement involves no injustice to the Schwendenerian school in Germany, who have done excellent service to anatomy, but from quite a different point of view. The question which they have attacked is that of the functions of the tissues, and the classification at which they have arrived, as fully set forth, ten years ago, in Haberlandt's *Physiological Anatomy of Plants* (2), is purely physiological. Such a classification, though just as important as a morphological one, is no substitute for the latter (*cf.* Strasburger, 3). What we want, as morphologists, is to trace the homologies of the tissues, and to find out, so far as we can, how the various forms and systems of tissue have arisen in the course of descent. That they have been modified at every step, and in every direction, in relation to changing function, is obvious; we cannot, however, be content with an arrangement based solely on the functions which they happen to discharge at the present stage of evolution.

Van Tieghem's anatomical system is best understood

if we remember that he started with the consideration of the *root*. Apart from more special researches, his first comprehensive work on general anatomy was devoted to this organ (4), and to that work our knowledge of the comparative structure of the root (so wonderfully uniform in its main features) in the various classes of vascular plants, is very largely due. The work in question, though intended to treat generally of the "symmetry of structure" in vascular plants, never advanced beyond the root, a fact which invited criticism. It has, however, borne abundant fruit in later years. Broad views on anatomical questions are likely to be attained by those who begin with the investigation of entire organs, rather than of special forms of tissue, and the fact that the organ chosen by Van Tieghem was one in which the systems of tissue stand out more clearly than in any other was fortunate for botany. In his *Treatise on Botany* (5), Van Tieghem keeps to the same order, and describes the structure of the root before that of the stem and leaf.

The characteristic feature of the anatomical teaching of Van Tieghem and his school, is the recognition of the *central cylinder* or *stele*, as a definite region comparable to cortex and epidermis. In the root the arrangement is clear enough. The central cylinder is perfectly well defined, including within it the ring of alternating bundles of wood and bast, accompanied by a certain amount of parenchyma (conjunctive tissue), the outer layer of which constitutes the pericycle, or limiting layer of the stele, so important from its various generative activities. Surrounding this is the cortex, the inner layer of which is the endodermis, and on the outside of the cortex there is the piliferous layer, which however is not in all cases recognised by Van Tieghem as a morphologically distinct epidermis.

Now the French botanist applies just the same principles to the stem. Here also he distinguishes three regions, central cylinder, cortex, and epidermis (5, p. 737). The cylinder is made up of the vascular bundles, and of conjunctive tissue. The pith is the part of the conjunctive tissue which lies within the bundles, that lying between

them constitutes the primary medullary rays, while the conjunctive tissue outside the bundles is the pericycle, often less obvious than in the root, but characterised by the same power of forming new tissues and organs.

Now the views just explained are essentially different from those of De Bary. The latter author does not recognise the central cylinder in the stem at all (except incidentally, in discussing the apical development). In the case of the root he describes the central cylinder as a single vascular bundle (comparable therefore to any one of the numerous collateral bundles of a typical Phanerogamic stem). Hence there is no attempt to bring out the homology between root and stem, while in Van Tieghem's system the recognition of this homology gives the clue to the whole anatomy. The central cylinder of the root is beyond question directly continuous with that of the stem, and not with any single bundle. The individual xylem and phloëm-strands of the root are continuous with those constituting the vascular bundles of the stem, though of course differently arranged. The mistaken opinion that the central cylinder of the root is a single vascular bundle arose from the exaggerated importance attached to the idea of the vascular bundle. As a matter of fact such bundles, in the sense of conjoint "liberoligneous" strands, do not exist, as such, in the root; the groups of xylem and phloëm are here separate from one another. In order to force the root into the vascular bundle scheme it was necessary to make a single bundle of its whole central cylinder, and thus the entire morphology of the vascular tissues was obscured.

The limitation of the central cylinder in the stem presents much greater difficulties than in the root. This is owing, in the first place, to the intimate relation between the vascular system of the stem and that of the leaves. In the great majority of Phanerogams, and in some Cryptogams, all the vascular bundles are "common" to leaf and stem, so that every bundle in the central cylinder of the stem is directly continuous with one which passes out into a leaf. Hence at every node the continuity of the main stele is

interrupted, and, where the leaves are crowded, the limits of the stele as a whole may be difficult to recognise. This however, though a difficulty, is not an objection. The stele is none the less a distinct region in the stem because it possesses prolongations into the leaves.

In the internodes themselves, however, there is often a much less definite outer limit to the stele in the stem than in the root. The endodermis, which in the latter organ forms so characteristic an inner boundary to the cortex, is not always obvious in the stem, though often it is so. The same remark applies to the pericycle. In the root this layer was recognised by Nägeli and Leitgeb (under the name of pericambium) as long ago as 1867. The presence of a corresponding zone in the stem was first established by Van Tieghem in 1882 (6) and more fully demonstrated by Morot in 1885 (7). The limit between pericycle and cortex is of course difficult to recognise where the endodermis is not specially differentiated. Still, by studying the development, and by tracing the connection with the tissues of the main root, the boundary can be sufficiently well traced. In any case the occasional want of a sharp limit to the stele is no argument against its recognition as a distinct anatomical region, any more than the existence of decurrent leaves is an argument against the morphological distinction between leaf and stem.

Where the vascular bundles bend out from the stem into the leaf, they are accompanied by conjunctive tissue. The name *meristele* is now applied by Van Tieghem to a bundle or group of bundles entering a leaf, with their enveloping conjunctive tissue (8, p. 284). Thus we see that the stelic tissue of the whole plant is continuous through all its organs, root, stem, and leaf (*cf.* Strasburger, 3, p. 111).

So far reference has only been made to the typical stems of Phanerogams, in which there is a single central cylinder forming the direct continuation of that of the main root. This "monostelic" condition is constant in the embryonic stem of all vascular plants. In many vascular Cryptogams, however, and in two Dicotyledonous genera (*Gunnera* and *Primula*, section *Auricula*) the cylinder

divides up, above the hypocotyl, so that the transverse section shows a number of equivalent steles, each of which repeats in essentials the characters of the single central cylinder of the embryonic stem. This is the case (according to the interpretation of Van Tieghem and his school) in most Ferns, and in many Selaginellas, as well as in the exceptional Dicotyledons above mentioned (9 and 5). According to De Bary's terminology each stele of these polystelic stems is simply a single concentric bundle. The researches of Leclerc du Sablon (10), who has investigated the transition from root to stem in many Ferns, seem to show conclusively that here, at any rate, the strands in question are best regarded as steles. We must not, however, exaggerate the importance of the distinction. Every gradation can be found between the most obvious stele and a single vascular bundle. The transition can, for example, be traced in following the vascular system of the leaf in almost any Fern. We reach a point at which the structure of the stele is so simplified that it becomes a matter of indifference whether the term stele or bundle be used. Even in the stems and petioles of Marattiaceæ, where the steles are of large size, and must each be regarded as including a number of vascular bundles, we find a structure very different from that of the typical central cylinder of a monostelic stem ; for all the protoxylem groups are on the same side of the wood, being directed towards the centre of the organ. Here, and in many similar cases, the strand termed by Van Tieghem a stele, while representing much more than a single vascular bundle, does not really correspond in structure with the entire cylinder of an ordinary stem, though in other cases again, the correspondence is complete. Still, the fact remains, that in the plants in question the central cylinder of the embryo is not continued as such into the upper part of the stem, but divides up, or branches out, into a number of distinct stelic masses, which lie embedded in the parenchyma.

Polystely, though so remarkable a modification from an anatomical point of view, is not necessarily of any importance as a taxonomic character, for not only do closely

allied species differ as regards their polystely or monostely, but even in one and the same shoot we may find a transition from the one condition to the other, and back again (*e.g.*, *Gunnera magellanica*, *Nephrolepis*).

It is unnecessary to follow the modifications of polystelic structure in detail, as the views of Van Tieghem have lately been embodied in an English text-book (see Vines, 11).

Another departure from typical structure is described by Van Tieghem under the name of *astely* (5, p. 764), or more precisely, as *schizostely* (8, p. 285). This is characterised by the fact that the stele completely breaks up into the individual bundles, each surrounded by its own peridesm[1] and endodermis, the cylinder thus ceasing to exist as a whole. Typical examples are found in *Equisetum limosum*, and other species, in *Nymphæa*, aquatic species of *Ranunculus*, etc. The French anatomist now regards all species of *Equisetum* as essentially astelic (except of course in the embryonic stem). The apparently monostelic structure in *E. arvense*, etc., is explained as arising from a re-fusion of the distinct bundles, "dialydesmy" thus becoming converted into "gamodesmy" (13).

Astely, as Strasburger has pointed out (3, p. 312), is a weak point in Van Tieghem's system. There is really no essential difference between the monostelic and the astelic Ranunculi, for example, except the presence in the latter of a distinct endodermis around each bundle. It is unreasonable to regard the central ground-tissue of *R. aquatilis* as homologous with the cortex, while in *R. repens* it is a true pith. Strasburger's view that there is a single stele in all these cases, but that its limits have become indefinite in the aquatic species, seems to do less violence to the facts than the interpretation of Van Tieghem.

The importance attached to the endodermis as a limiting layer is in fact altogether exaggerated in Van Tieghem's system. The appearance of a differentiated

[1] *Peridesm* is the conjunctive tissue at the periphery of any portion of a subdivided stele, and is now distinguished from the *pericycle* which surrounds an entire stele (see Van Tieghem, 12).

endodermis around a vascular bundle has only a physiological significance, and cannot change the homologies of the surrounding tissues. Pfitzer (14) showed long ago that in some Equiseta mere strands of parenchyma may be surrounded by their own endodermis; yet no one would regard such strands as homologous with vascular bundles or with steles.

Van Tieghem, in his *Treatise on Botany*, has a section on the return from polystelic or astelic to monostelic structure (5, p. 767). In the pedicel of an Auricula, for example, the separate steles first become fused laterally, giving rise to the "gamostelic" condition, and next the internal phloëm, pericycle, and endodermis disappear, so that a typical monostelic structure is assumed. Here it is evident that the pith of the monostelic pedicel is the direct continuation of the "internal cortical tissue" of the polystelic part of the stem. The author warns us, it is true, against confusing such stems, which have become monostelic by reduction, with truly monostelic organs; there is, however, no distinction whatever in their structure. Cases such as these warn us that the theory of polystely must not be applied too rigidly, if we would avoid forced interpretations.

The fundamental question now arises: Does the distinction of the great anatomical regions, stele, cortex and epidermis, rest on a satisfactory *developmental* basis? Can we trace definite histogenetic layers, whether in the actual embryo, or in the growing points, from which, and from which alone, the definitive histological regions are respectively derived?

The whole subject of histogenesis is far too large to be treated of in this article; but some attempt may be made to indicate the present position of the question.

The great majority of plants differ from animals in being permanently embryonic: that is to say, they retain, throughout their whole life, portions of undifferentiated tissue in their growing points, which continue to give rise to new organs and new tissues, until the life of the whole plant is at an end. Exceptions to this rule are very rare. Ludwig

Koch has recently shown (15, p. 452) that in some lilies the growing point of the stem altogether perishes, at a time when the stem is scarcely half-grown, so that the later stages of development, so far as that particular stem is concerned, are gone through in the absence of any embryonic tissue. If the same thing occurred in an entire plant, as may perhaps be the case in some annuals or other monocarpic plants, we should have a mode of development in which the whole of the embryo becomes converted into permanent tissue. In plants generally, however, the reverse is the case. The embryo, as it exists in the seed, does not *directly* give rise to the entire organism, but embryonic development is continued in every separate growing point. Hence, perhaps, it is that the study of embryology, in the ordinary sense, has led to less important results in the vegetable than in the animal kingdom, though this fact is also due to the extreme meagreness of our present knowledge of the embryology of plants.

The application of the term *embryonic tissue* to the meristem of all growing points, which has become so general of late years (*cf.* Sachs, 16, p. 503), is not without its dangers. We must not suppose that the meristem of a growing point, in a plant at an advanced stage of development, is really identical with the meristem of the actual embryo. Suppose, for example, to take a concrete instance from our immediate subject, that we wished to determine whether, in a particular case, the polystelic or the monostelic condition of the stem was the more primitive. The study of the growing points in an advanced specimen of the plant would not help us. We should find that if the branch were polystelic so would be its growing point also; from the earliest differentiation of tissues at the apex, the full number of steles would be present. This is the case even in a genus like *Gunnera*, in which we have every reason to believe that polystely is a recently acquired character. Only by having recourse to the embryo itself, to the first-formed parts of the stem, could we satisfy ourselves that monostely precedes polystely in the course of development. The same principle holds good throughout. A given

growing point, under normal conditions, is only adapted to produce the particular order of branch to which it belongs ; its development does not necessarily throw any light on the way in which the structure first arose. Hence the theory of recapitulation, however true of the development of the plant as a whole, need not apply at all to the apical development in a plant which is no longer an embryo. To the neglect of this distinction we may perhaps attribute the fact that the recapitulation-theory has never been adequately tested in the vegetable kingdom.

In considering the subject of histogenetic layers, we have therefore to distinguish two questions : (1) Do such layers exist in the embryo? (2) Can they be traced in the various growing points, at any stage of growth ?

As regards the former question, it is probable, judging from the comparatively few cases which have been investigated, that in the main axis of the embryo such a differentiation, into plerome, periblem and dermatogen, giving rise to stele, cortex, and epidermis, does exist (see, besides the older literature, Chauveaud, 17). There is no need to dwell on these phenomena, which have been familiar to botanical students for many years ; we can only wish that the conclusions rested on a somewhat broader basis. There is, however, a fatal objection to anything like a theory of "germinal layers" in plants, as was clearly pointed out by De Bary in his criticism of Famintzin (1, p. 23). The plerome of the embryo may give rise to the whole vascular system of the main axis, but it certainly has nothing to do with the origination of the same system in the lateral appendages of the stem. Such appendages are of exogenous origin, and there is no doubt that their vascular tissues, like all their other tissues, arise from the *outer* layers of meristem of the parent organ, and not from its plerome. The converse case occurs in the root, where the appendages are endogenous. In Phanerogams all the tissues of a lateral root arise from the pericycle, and thus belong to the plerome of the main root, while in vascular Cryptogams they arise from the endodermis, and are thus all alike of cortical origin. Hence it is impossible to derive

the tissues of the entire plant from special germinal layers.

Passing on to the question of histogenetic layers in the apical meristem of growing points, the problem is, of course, different for plants which grow by means of a single apical cell, and for those which have a small-celled apical group. In the former case, as the apical cell gives rise to all tissues alike, the question is whether the early division walls of the segments mark out the boundaries between the principal systems of tissue. Certainly the mere order of succession of the walls has no constancy. Thus, according to Van Tieghem, while in most Ferns the first tangential septum marks the limit between inner and outer cortex, and the second defines the central cylinder, in *Osmunda* and *Todea* the order is reversed (18). In the former case endodermis and pericycle are more nearly related to one another than are the two layers of the cortex. From this and numerous similar instances it is evident that the order of appearance of the cell-walls does not correspond with the relative importance of the subsequent differentiation.

In a monostelic stem (*e.g.*, a stolon of *Nephrolepis*), the first tangential wall corresponds to the outer limit of the central cylinder. In polystelic stems of other Ferns the same wall marks the outer limit of the ring of steles, but here a great part of the tissue to the inside of this wall forms part of the cortex, according to Van Tieghem's use of the term. It is evident that in this case the cortical tissue between the steles is of common origin with the steles themselves, yet the distinction between these regions is, according to Van Tieghem, the most fundamental in anatomy. It may seem trivial to dwell on such minutiæ of development, but we must remember that in other cases the most important morphological distinctions have been based on equally slight differences of origin. Thus in the development of the root, the piliferous layer arises in nearly all Dicotyledons from the outermost part of the meristem, and is thus of common origin with the root cap, while in the Monocotyledons generally, the piliferous layer is derived

from the middle meristematic group, and is thus of common origin with the cortex. On this distinction Van Tieghem (5 and 19) bases the statement that the root of most Monocotyledons has no epidermis, while that of Dicotyledons possesses a true epidermis. The strict application of such a principle would surely lead to very inconsistent results.

Returning to plants with an apical cell, we may safely conclude that, with the solitary exception of the origin of the root cap, the first tangential cell-walls do not constantly mark out the main histogenetic layers, from which the principal system of tissues is derived. Such layers arise in different ways in different cases, and at various distances from the apex.

As regards the Phanerogams, the researches of Van Tieghem and Douliot (19) appear to show that in roots, at any rate at their first origin, the histogenetic layers are really distinct. We may be permitted to doubt whether they always remain so throughout the entire development.

The question as to the apex of the stem in flowering plants is really in a state of chaos, and we cannot claim to have made any advance since the days of Sanio and Hanstein. A paper published by Douliot in 1890 (20) attempted to set the matter at rest. He adopted the view that in Gymnosperms, as in most vascular Cryptogams, there is a single apical cell, but his figures fail to support this conclusion. Among the Angiosperms, Douliot found in Dicotyledons generally the three initial layers required by the theory of Hanstein, while in most Monocotyledons, he reduced them to two, plerome and periblem having a common initial group.

Almost simultaneously with Douliot's paper a work appeared by Ludwig Koch on the apex of the stem in Gymnosperms (21). He decides against the existence of an apical cell, and his figures, drawn from microtome sections, appear to be quite conclusive on this point. Just as little, however, does he support the theory of histogenetic layers. He finds, in agreement with previous observers,

that even the dermatogen is not constantly distinct in Gymnosperms, the outermost apical layer undergoing periclinal divisions and thus contributing to cortex as well as to epidermis. Neither does Koch find any distinct initial group for the central cylinder; on the contrary, he finds that the supposed plerome gives rise to the pith only, which is the first tissue to be differentiated, the procambial strands arising from the so-called periblem. This is to some extent a return to the views of Sanio and Russow.

A more recent paper by Koch, already referred to (15), is of great interest from this, as well as from other points of view, and deals the heaviest blow at the theory of Hanstein which it has yet received. So long as the evidence against this theory was merely negative, depending on the frequent impossibility of distinguishing the histogenetic layers at the apex, it was always open to its supporters to argue, with Van Tieghem (5, pp. 697 and 776), that our conclusions must be drawn from the cases where the layers are distinct, and that their want of distinctness in other cases is merely due to displacements of the initial cells.

The special merit of Koch's work consists in the fact that he has compared similar growing points of the same plant, at different stages of development of the shoot. He finds that while, in Angiosperms, the dermatogen remains distinct, the number of layers of "periblem" varies according to the stage of growth. Thus in the lilac (15, p. 401), if a growing point be examined between two periods of leaf-formation, a single initial layer of "periblem" is found. When, however, a new pair of leaves is in process of origination, this layer undergoes periclinal divisions into three or four layers, the apparent limit between periblem and plerome becoming less distinct. At the next phase, when the origination of new leaves has ceased for the moment, we find the one initial layer again, and so on. Similar results were obtained in *Berberis* (15, p. 412). Now it is evident that this variation in the number of apparent periblem-layers, proves that they are not permanently distinct as histogenetic initials. The same layer

which at one time gives rise to cortex must at another phase of growth contribute to the plerome. Koch's observations seem fatal to the theory of histogenetic layers as regards the plants in question.

In Angiosperms, as well as in Gymnosperms Koch believes that Hanstein's plerome is nothing but the young pith (15, pp. 394, 401). He does not, however, find a distinct initial group, even for the pith, though that is the first tissue to become recognisable. According to his views, the embryonic tissue of the apex grows as a whole, and neither the pith nor any other tissue has any need of a special histogenetic layer.

The conclusion which appears to follow from our present knowledge of the development of growing points, is that the lines of cell-walls nearest the apex do not necessarily mark the limits of the most important tissue-systems. As Sachs long ago showed, the direction of the cell-walls is quite independent of the nature of the organ; a young hair and a young embryo may present an exactly similar arrangement of their cells (22).

In *Caulerpa* differentiation goes on without the formation of any cell-walls at all; in such Algæ as *Stypocaulon*, though segmentation occurs, morphological differentiation (branching) begins in the apical cell itself, before any cell-walls have been formed, while in the closely allied *Sphacelaria* the reverse is the case. So too with the internal morphology; the systems of tissues may begin to differentiate at a greater or less distance from the actual apex; their boundaries may or may not follow those lines of cell-walls which happen to be first formed.

An interesting paper by Flot (23) has recently appeared, bearing on the differentiation of the tissues. The paper is written from the point of view of Hanstein and Douliot, but this does not essentially affect his main results. Flot's chief conclusion (based principally, though not exclusively, on the investigation of Dicotyledons) is that there is a definite "perimedullary zone" within the ring of vascular bundles, corresponding in many respects to the pericycle at their outer margin. This perimedullary zone is quite distinct

from the pith itself. The cells from which the central cylinder is derived, divide by lateral (periclinal) walls to form the "vascular meristem," and by walls at right angles to the axis (anticlinal) to form the true pith, or inner conjunctive tissue. The "vascular meristem" of Flot gives rise to the vascular bundles and to the outer conjunctive tissue, which includes the pericycle, the primary rays, and the perimedullary zone.

Where the bundles remain separate, the perimedullary zone only becomes differentiated on the inner side of each individual bundle; where they are united to form a continuous ring this zone forms a continuous ring also. It may become wholly or partially converted into sclerenchyma, just as is the case with the pericycle, and like the latter may be the seat of various new formations. Thus the internal phloëm which occurs in Cucurbitaceæ, Solanaceæ, and many other Dicotyledous orders, is derived from the perimedullary zone, and so is the internal wood and bast found around the pith in species of *Tecoma*, *Rumex*, etc. It appears therefore that internal phloëm belongs neither to the vascular bundles themselves, nor to the true pith; it arises from this intermediate zone, which had not been distinguished by previous observers.

Flot's figures appear to show that his perimedullary zone or internal pericycle, is really distinct, differing from the pith both in its origin and in the form of its cells.

Van Tieghem, in his *Traité de Botanique* (5, p. 763), calls special attention to stems with *perixylic* structure, or in other words, with centripetally developed primary wood. Perixylic structure is characteristic of all roots, and of the first stage of the stem (hypocotyl) in most vascular plants; it persists throughout the whole of the stem in most vascular Cryptogams, while *centroxylic* structure (centrifugal primary wood) is general in the epicotyledonary stem of all Phanerogams, and of some vascular Cryptogams, as *Equisetum* and the *Ophioglosseæ*. This distinction is certainly one of the most important in anatomy, and it appears probable that perixyly is the more primitive condition. The peculiar structure of the foliar bundles in

Cycads appears to be a case of the persistence of perixyly. Remarkable transitions between Cycadean and Filicinean anatomy are found in some carboniferous fossils, such as *Lyginodendron.*

A recent paper by Poirault on the "Anatomy of Vascular Cryptogams" (24) contains several points of interest, many of which however have been anticipated in his preliminary notes. Poirault gives for the first time a full account of the anatomy of the stem in Gleicheniaceæ, which are monostelic Ferns with a somewhat complex central cylinder, the protoxylem-groups lying at or near the periphery of the wood. His description suggests a considerable analogy with certain fossil forms.

Poirault has discovered in the leaves of certain Ferns, such as *Acrostichum*, a tissue which appears to correspond to the tracheal transfusion-tissue, which supplements the xylem of the vascular bundles in the leaves of Conifers and Cycads.

This author has investigated the changes undergone by the steles of ferns in their passage into the leaves. As Haberlandt previously found, they ultimately assume a collateral structure owing to the disappearance of the upper part of the phloëm. Poirault finds that this change may take place in the petiole, or at the base of the lamina, or only in the fine branches of the bundles within the latter.

Another important contribution to our knowledge of the structure of vascular Cryptogams is Harvey Gibson's paper on the Comparative Anatomy of *Selaginella* (25), which for the first time enables us to form an adequate idea of the range of anatomical variation within that remarkable genus. It is impossible to summarise his work in this article, but attention may be called to his interesting observation that in the basal portion of the stem of *Selaginella spinosa*, the protoxylem of the stele is central not peripheral, showing that perixyly is not absolutely constant in the genus.

A short paper by Petersen (26) is of interest as showing that a regular gradation can be traced from Monocotyle-

dons with primary tissue-formation only, to those with a definite zone of secondary thickening. To the latter category the shrubby Irideæ must now be added (27).

Before concluding this article some mention must be made of Schenk's important work on the anatomy of Lianes (28). This is a very full account based on original observation, and abundantly illustrated, of the highly modified structure of climbing plants. The author finds that habit and anatomy do not necessarily go together; thus twiners and tendril-climbers, so different in their external characters, have their most characteristic anatomical peculiarities in common.

The author is not contented with merely describing the anomalous structures, but also endeavours to trace their origin. In each case there are three possibilities to be considered: the character may be inherited, or may be the direct result of the action of external conditions, or may be adaptive. That the direct action of the conditions of growth has a considerable influence is shown by the fact that cultivated specimens of anomalous climbing plants, grown in the absence of supports, may develop an almost normal structure. Hegler's remarkable experiments on the development of sclerenchyma in consequence of increased tension, throw some light on this subject. There is, however, no evidence that such directly acquired characters can be inherited, though Schenk seems to assume that this may happen.

The author shows that the most modified members of very different families may present quite similar adaptations. Illustrations of this are afforded by the comparison of *Bauhinia* with climbing Malpighiaceæ, of *Bignonia* with *Phytocrene*, or of *Wistaria* with the anomalous Menispermaceæ. Few fields of investigation offer more striking examples of homoplastic characters.

In those who believe that all characters whatsoever are, or have been adaptive, the distinction between inherited and adaptive characters cannot be an absolute one. The distinction is between recently acquired adaptations, and those which arose in the more remote past, and have come

to be the common property of considerable groups of plants.

For example, the appearance of extrafascicular cambium, resulting in the formation of successive rings of bundles, in certain Leguminosæ and Menispermaceæ, and in *Antigonum* among the Polygonaceæ, is presumably a relatively recent adaptation, correlated with the climbing habit; extrafascicular cambium in Amaranthaceæ, however, is a family character, acquired at some long past epoch and no longer of obvious adaptive value. The inherited character has, however, been somewhat further developed in some members of the family which have become climbers. Here an "inherited" character has again become adaptive. On the other hand we sometimes find a member of a climbing order, which is not itself a climber, and yet has anomalous structure. This is the case with *Cocculus laurifolius* (28, p. 64), a Menispermaceous tree, which has concentric rings of bundles. If, as Schenk suggests, this plant is descended from climbers, it affords a good example of a purely adaptive modification which has become an inherited anatomical character.

Schenk's book is full of interesting details, which, unfortunately, there is no space to discuss.

It is superfluous to make any apology for the obvious incompleteness of this article. Only a small part of the field of internal morphology has been touched on at all, and that only in the most cursory manner. It is hoped, however, that enough has been said to show that the study of the internal structure of plants, if freed from the grooves of mere description, and carried on from the point of view of descent and adaptation, is likely to yield just as valuable results as that of external morphology.

BIBLIOGRAPHY.

(1) DE BARY. *Comparative Anatomy of the Phanerogams and Ferns*, English translation, 1884.
(2) HABERLANDT. *Physiologische Pflanzenanatomie*, 1884.
(3) STRASBURGER. *Histologische Beiträge*, bd. iii., 1891.

(4) VAN TIEGHEM. Recherches sur la Symmétrie de Structure dans les Plantes Vasculaires. I. La Racine. *Ann. des Sci. Nat.* (*Bot.*), sér. 5, t. xiii., 1871.

(5) VAN TIEGHEM. *Traité de Botanique*, second edition, 1891.

(6) VAN TIEGHEM. Sur quelques points de l'anatomié des Cucurbitacées. *Bull. de la Soc. Bot. de France*, t. xxix., 1882.

(7) MOROT. Recherches sur le péricycle. *Ann. des Sci. Nat.* (*Bot.*), sér. 6, t. xx., 1885.

(8) VAN TIEGHEM. Sur la structure primaire et les affinités des Pins. *Journal de Botanique*, t. v., 1891.

(9) VAN TIEGHEM and DOULIOT. Sur la polystélie. *Ann. des Sci. Nat.* (*Bot.*), sér. 7, t. iii., 1886.

(10) LECLERC DU SABLON. Recherches sur la formation de la tige des Fougéres. *Ann. des Sci. Nat.* (*Bot.*), sér. 7, t. xi., 1890.

(11) VINES. *Students' Text-book of Botany*, pt. i., 1894.

(12) VAN TIEGHEM. Péricycle et péridesme. *Journal de Botanique*, vol. iv., 1890.

(13) VAN TIEGHEM. Tige des Prêles. *Journal de Botanique*, vol. iv., 1890.

(14) PFITZER. Die Schutzscheide der deutschen Equiseten. Pringsheim's *Jahrbuch für wiss. Bot.*, bd. vi., 1867.

(15) LUDWIG KOCH. Die vegetative Verzweigung der höheren Gewächse. Pringsheim's *Jahrbuch für wiss. Bot.*, bd. xxv., 1893.

(16) SACHS. *Vorlesungen über Pflanzen-physiologie*, first edition, 1882.

(17) CHAUVEAUD. Recherches embryogéniques sur les laticiféres des Euphorbiacées, etc. *Ann. des Sci. Nat.* (*Bot.*), sér. 7, t. xiv., 1891.

(18) VAN TIEGHEM. Limite de l'écorce dans les Cryptogames vasculaires. *Journal de Botanique*, t. ii., 1888.

(19) VAN TIEGHEM et DOULIOT. Origine des Membres endogénes. *Ann. des Sci. Nat.* (*Bot.*), sér. 7, t. viii., 1889.

(20) DOULIOT. Croissance terminale de la tige. *Ann. des Sci. Nat.* (*Bot.*), sér. 7, t. xi., 1890.

(21) LUDWIG KOCH. Bau und Wachsthum der Sprossspitze. I. Die Gymnospermen. Pringsheim's *Jahrbuch für wiss. Bot.*, bd. xxii., 1891.

(22) SACHS. Anordnung der Zellen im den jüngsten Pflanzentheilen. *Arbeiten des bot. Instituts im Würzburg*, bd. ii., 1880.

(23) FLOT. Recherches sur la zone périmédullaire. *Ann. des Sci. Nat.* (*Bot.*), sér. 7, t. xviii., 1893.

(24) POIRAULT. Recherches anatomiques sur les Cryptogames vasculaires. *Ann. des Sci. Nat.* (*Bot.*), sér. 7, t. xviii., 1894.

(25) HARVEY GIBSON. Contributions towards a knowledge of the Anatomy of Selaginella. I. The Stem. *Annals of Botany*, vol. viii., June, 1894.

(26) PETERSEN. Remarques sur la croissance en épaisseur et sur les regions anatomiques de la tige Monocotylédone. *Botanisk Tidsskrift*, bd. xviii., 1892. French Résumé.

(27) SCOTT and BREBNER. On the Secondary Tissues in certain Monocotyledons. *Annals of Botany*, vol. vii., March, 1893.

(28) H. SCHENK. Beiträge zur Biologie und Anatomie der Lianen. II. Theil. Schimper's *Bot. Mittheilungen aus den Tropen*, Heft 5, 1893.

D. H. SCOTT.

THE ARRANGEMENT OF THE MOLECULES IN A CRYSTAL.

ALTHOUGH we have little or no certain knowledge about the molecular structure of solids in general, some very interesting investigations have recently been made into the nature of *crystalline* structure. Little has been published on the subject in England, and it is more than probable that these speculations have escaped the notice of many even of those who are both familiar with the theory of gases and take an interest in recent speculations on the nature of liquids.

With the object therefore of briefly summarising, at any rate some of the more important points which have been established in this fascinating subject: I propose first to indicate what are the views to which crystallographers have been led by more purely mathematical reasoning, and in a subsequent article to criticise the applications of the geometrical theories of crystalline structure which have been made by those who have attempted to give them a physical meaning. To this category belong the interesting structures devised by Lord Kelvin, Liveing, Barlow and Beckenkamp.

In order to be of any practical use, such a theory should enable us to say something about the arrangement of the particles which constitute a crystal, or about their symmetry, sufficient to explain the form and properties of a given substance in a consistent manner. The attempt to define the actual arrangement of the crystalline particles has as yet been made for very few compounds, but that it has been made, and with a certain show of success, will be evident from a few instances.

In the first place, Sohncke in 1888 (1) devised an arrangement which should accord with the physical and morphological features of quartz, by supposing a spiral line drawn upon the surface of a circular cylinder, and placing particles at equal intervals along this line, so that the fourth comes vertically above the first, the fifth above the second, and so on, the particles thus lying upon the thread of a screw.

This is next converted into a "compound screw" by placing another equal and similar screw of particles inside the first, so that their axes coincide, but not their threads; and, finally, a number of these compound screws are ranged beside one another in parallel position, so that their axes (supposed vertical) would pass through the knots of a horizontal net whose meshes are all equilateral triangles. This somewhat complicated arrangement is practically identical with that adopted by Wulff (2) and does not differ essentially from one proposed by Mallard.

Three different authors are therefore in agreement upon a possible grouping of the particles in at least one substance.

Probable structures have subsequently been suggested for many substances by Mallard, Wulff, Sohncke, Liveing, Fedorow, Lord Kelvin and others, but they need not be quoted since our purpose is only to show at present that the subject is now regarded as something more than a geometrical problem, and a few instances will suffice.

Another step is taken by Muthmann (3), who, in a recent paper on the alkaline phosphates and permanganates, comes to the conclusion that the potassium phosphate KH_2PO_4 is constructed of "physical molecules," arranged in the following manner: Space having been divided into equal square prisms, of which the height is represented by the number 2·9503, and the horizontal diagonal by 4·4432, a molecule is placed at the centre and at all the corners of each prism; in the corresponding arsenate the arrangement is supposed to be the same, but the prism has a height of 3·0235, and diagonal 4·5583. These numbers are taken to indicate the comparative molecular intervals along the two corresponding directions in the two salts, a calculation which has never previously been made for any substance. These intervals would be different for the two salts even if they had absolutely the same crystalline form, so it is clear that they are not identical with the usual axial ratios of the crystallographers.

The author goes even further; he asserts that each "physical molecule" consists of eight chemical molecules KH_2PO_4, and he supposes that in this complex cluster

eight radicles $PO(OH)_2$ are arranged at the corners of two superposed horizontal squares, and that vertically above or below each of these is attached a radicle OK.

Tutton (4), in a paper read quite recently before the Chemical Society, has independently adopted precisely the same reasoning, and feels himself justified in supposing that in rubidium sulphate (for example) the crystal elements are situated at the corners of a rectangular rhombic prism of certain dimensions, and that each of them is composed of four chemical molecules arranged in a certain symmetrical manner.

Yet further: the test by which any theory must stand or fall as a working hypothesis is the extent to which it renders prediction possible; and the relations between form, structure and composition have at length been so far ascertained, that the pioneers in this study already venture to predict the form of new or undetermined compounds.

Thus Muthmann states that rubidium sulphate will doubtless be found to have a form almost identical with that of thallium sulphate, and within a few months this prediction has been fully verified by Tutton's measurements.

To take another and a somewhat different example, Penfield (5), in a recent paper on the Chondrodite group of minerals, uses the following words: "Thus $Mg[Mg(F,OH)]_2SiO_4$ is a possible and most likely compound to occur. This should crystallise either orthorhombic or monoclinic with $\beta = 90°$, and should have the axial ratio $a : b : c = 1{\cdot}086 : 1 : 1{\cdot}887$." Here we have a prediction of the crystalline form of an absolutely unknown compound, and one in this case not based in the least upon considerations of isomorphism.

We hope to return to these and other instances in a subsequent communication; for the present, we have to see what is the geometrical reasoning upon which the current theories of crystal structure are founded.

There is some danger lest assertions like those of Muthmann should be either accepted without reservation as

established facts, or dismissed as mere fanciful speculations, by readers who have not had occasion to follow the growth and development of the theories upon which they are based. They are, as we shall see, neither the one nor the other.

THE LATTICE THEORY.

Omitting the somewhat crude theories of earlier investigators, the first serious attempt to grapple with the theory of crystal structure is the work of the French mathematician, Bravais (6). His classic memoir on Systems of Regularly Distributed Points is an investigation of all the possible forms of space-gratings (Réseaux).

The reader may get a fair idea of what is meant by a space "grating," or "lattice," or (as I have previously termed it) (7) a "space-network," by imagining a hailstorm to become suddenly rigid, each hailstone being arrested at a given instant, and fixed at the point of space which it occupies at that moment. Imagine also that the hailstones were descending at perfectly regular intervals, so that when the whole set becomes fixed the hailstones lie at equal intervals upon parallel straight lines like beads strung upon straight wires; if there is no wind these lines are vertical.

Suppose, further, that the lines of descending hailstones are separated by certain regular intervals in such a way that they lie in planes, that all the lines lying in one vertical plane are equidistant, and that in all parallel planes the distance between adjacent lines is the same. It will now be clear that the hailstones not only lie in vertical lines and planes, but also in horizontal or transversely inclined lines and planes; such that along all parallel lines the particles lie at equal intervals, and in all parallel planes the lines which run in the same direction are separated by equal intervals.

A hailstorm of this nature, supposed to become rigid, constitutes a space-lattice or network of particles of the sort investigated by Bravais.

The lattice-structure is one which may very reasonably

be ascribed to a crystal, for it harmonises both with the geometrical and physical characters.

The line joining any two particles is a line having a whole series of particles strung upon it at equal intervals along its whole length; every parallel line of particles is identical with it; but along other lines the molecular interval is in general different. In such a structure we should expect the physical properties (elasticity, thermal expansion and conductivity, etc.) to be the same along all parallel lines, and different in general along lines which are not parallel; now these are precisely the characters exhibited by a crystal.

Again, the plane through any three particles is such that it is filled with particles arranged like the knots upon a net, and every parallel plane is exactly similar to it. If such a structure is bounded by plane faces, we should expect them to be just these planes, or (as I have previously called them) "webs" of particles. It will be found that all these planes are related to one another in such a way that, if we know the directions of any four, we can predict with absolute certainty the directions of all other planes which may be constructed out of the lattice. Now this relation is precisely that which governs the mutual inclinations of a crystal's faces, known to crystallographers as the Law of Rational Indices.

Or we may regard the structure in another way. It is clear that the nature of the whole lattice is completely defined by one of the cells or parallelepipedal spaces enclosed by six adjacent particles. Thus, returning to the hailstorm illustration: if the hailstones are descending in vertical lines in a still air; and if the distance between two adjacent lines is equal to that between two consecutive hailstones in the same line; and if adjacent hailstones are on the same horizontal level; then the cell formed by six adjacent particles may be a cube, and the whole lattice may consist, as it were, of particles placed at the corners of cubes packed in side by side so as to fill space; a sort of arrangement like that of a wire egg-rack.

The cube being then one possible form, we may next

alter the shape of the elementary cell in various ways by supposing the distances between the lines to be unequal, or by imagining the lines of descending hailstones to slope in one direction or the other as they would do under the influence of a wind. In this way we may obtain different cells having the forms known as the square prism, the oblique prism, the rhombohedron, etc., of crystallography, and a different lattice corresponding to each of these.

Now the most important and remarkable of all the characters of a crystal is its symmetry. As is well known, crystals are almost always symmetrical in shape, and according to the nature of their symmetry they are grouped into seven types or "systems". The grouping is sometimes expressed according to the lengths and directions of the three axes to which a crystal is referred; but it will be seen on reflection that these axes are nothing more or less than the three sides of the elementary cell of the lattice. A variation of the lengths and directions of the axes corresponds exactly to a variation of the form of the elementary cell, and by either process we obtain geometrical figures, which generally possess a certain symmetry.

Bravais, therefore, proceeded to investigate all the possible forms of space-lattices and the nature of their symmetry; and the result at which he arrived is surprisingly simple.

He found that there are only fourteen different sorts of lattices, and that these, if classed by their symmetry, fall into seven groups, which have precisely the same symmetry as the seven systems into which all known crystals had already been grouped.

The lattice structure, however, fails to explain the form of what are commonly called hemihedral or merohedral crystals, *i.e.*, those which only partially possess the symmetry of the system to which they belong; it gives us, for example, the form of the cube and octahedron of fluor-spar and other substances, but it does not account for the form of the tetrahedron of zinc-blende, or the common deltoid dodecahedron of iron pyrites. For all these merohedral forms (and they perhaps constitute in reality the majority of crystals),

Bravais was compelled to introduce another assumption; the *particles* themselves were supposed to have an inherent symmetry; to be, not mere points, but symmetrical clusters of atoms; the symmetry of the crystal is then supposed to be, not that of the lattice alone, but that which is common to the lattice and to the molecules of which it is constructed. Consequently, if the molecular cluster is less symmetrical than the lattice, the crystal as a whole only possesses a part of the symmetry of the system to which it belongs.

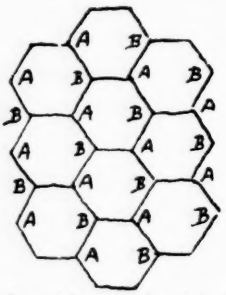
For example: minute cubic particles placed at the corners of a cube would be the elementary cell of a completely cubic lattice like that of fluor-spar; but minute tetrahedra placed at the corners of a cube would give rise to a *hemihedral* cubic crystal like zinc-blende.

This is, of course, all ancient history now, but it is necessary for the correct appreciation of what follows. We may note in passing that it is difficult to see why in the Bravais theory each molecule, if its symmetry depends upon the arrangement of atoms or chemical molecules within it, should necessarily have only a symmetry deducible from the lattice to which it belongs. Why, for instance, should the molecule not be pentagonal in form, although this is not admissible as a crystal form? This objection cannot be raised to the second theory, which we have now to consider.

THE SOHNCKE THEORY.

The next advance, and a very important one, was made independently by Wiener and Sohncke, who endeavoured to solve a different problem, namely: In how many ways can a set of points be arranged in space so that the distribution round any one point is identical with that round any other of the set? In other words: How many possible *homogeneous assemblages* of points can be constructed? This is a very natural structure to assume, for it harmonises with the homogeneous physical nature of a crystal; the assumption made is, that the symmetry at any one point of the structure is the same as at any other point.

In the Bravais theory it was assumed that all the particles are parallel, and with this limitation it is true, as was shown by Sohncke, that the lattice is the only possible homogeneous assemblage. But why are the molecules to be necessarily parallel? Why, for instance, should not a bees-cell structure like the adjoining figure be possible?



This cannot form any part of a lattice, both because the particles do not lie at equal distances along straight lines; and also because particles at A are not parallel to particles at B; for A has its three nearest points lying horizontally to the left, and both above and below it to the right, whereas B has its three nearest points lying horizontally to the right, and above and below it to the left; A is in fact identical with B turned through 60°.

Sohncke then investigated (8) all the possible homogeneous assemblages of points, and came to the conclusion that there are sixty-five (the number was first erroneously given as sixty-six); further, that these, if arranged by their symmetry, fall into groups which agree with the various modes of symmetry observed in crystals, including almost all the merohedral types.

Among these sixty-five structures there are a number of remarkable spiral and alternating arrangements (*e.g.*, that suggested above for quartz) which seem to have no relation to Bravais lattices; further, all the possible modes of crystalline symmetry seem to be explained by the arrangement of similar particles identically grouped throughout the mass; only with the implied possibility that the particles need not be similarly orientated, but may face different ways.

Sohncke's theory seems then to be free from the arbitrary assumptions of the Bravais theory, and to be totally distinct from it.

There is, however, a closer connection between the two theories than is at first apparent; for it can be proved that each of Sohncke's sixty-five structures consists of two or more interpenetrating lattices inserted into one another.

For example, the bees-cell arrangement of the figure may be regarded as two interpenetrating webs with triangular meshes, one formed by the points A, and the other by the points B; and one of these is the same as the other turned through 60°.

Hence a cluster of adjacent points belonging to the different lattices in this new theory corresponds to a molecular cluster forming a particle in the lattice of Bravais. None the less, however, the newer theory is a remarkable advance upon the old, because the symmetry is deduced in a general way from the arrangement of the particles without any auxiliary hypothesis.

A few years ago, however, Wulff (9) made the curious discovery that one type of symmetry, namely, that characteristic of the rare copper silicate dioptase, is not to be found among the structures of Sohncke. Moreover, among the types of partial symmetry some of the *hemimorphic* or polar forms, such as those of tartaric acid and struvite, cannot be explained by Sohncke's theory without some assumption regarding the form of the particles themselves; such an assumption was made by Sohncke, and is just as much an objection to his theory as the corresponding assumption made by Bravais to account for all the merohedral types.

It, therefore, became necessary for Sohncke to remodel his system so as to include the missing type, and he did so by an assumption which also explains the hemimorphic forms without any auxiliary hypothesis. This principle, which had been previously employed by Barlow (10), consists in supposing two or more *different sorts* of particles to enter into the crystal structure.

The interpenetrating lattices which constitute a crystal are no longer identical points, or merely the same lattices turned in different directions, but one consists of particles of one sort, and another may consist of particles of a different sort; this limitation will evidently reduce the symmetry of the composite lattice, and can be made to include all possible varieties of crystals.

Here, then, we begin to have a suspicion that crystalline symmetry is not a matter of mere geometry, and to recognise

the necessity of taking into account the nature of the particles; in other words, the chemical structure of the substance.

By this time, however, we seem to have lost sight of the simple form of a crystal, in order to account for its symmetry; the composite interpenetrating lattices and spirals of Sohncke may show every conceivable variety of crystalline symmetry, but they do not lead to simple plane faces which obey the law referred to above as the law of rational indices. In order to limit the faces to such as obey this law, we have to suppose that the only planes possible as crystal faces are those which belong to any one of the lattices contained in the structure.

It appears, then, that in Sohncke's theory a cluster of particles corresponds exactly to a molecule in the Bravais theory; the crystal faces and the planes of cleavage are any planes containing a set of these molecules; and the symmetry of the Bravais molecule is that of the Sohncke cluster, and upon this in reality the symmetry of the crystal (and of the lattice) depends.

At this stage we have arrived at the following conclusions: If the orderly arrangement of particles is to be taken to represent the structure of a crystal, then, in order to account for the crystal faces we require a lattice structure; in order to account for the symmetry we require a homogeneous assemblage; the latter turns out to consist of interpenetrating lattices; but we have further to suppose the lattices to be different in their nature.

THE THIRTY-TWO TYPES.

Meanwhile the problem had been approached in a different manner. Including all the merohedral forms, it will be found that the total number of varieties of symmetry possible in crystals amounts to thirty-two; these, of course, naturally fall into seven groups identical with the seven systems of crystallography.

It has been only recently pointed out (11) these thirty-two groups were established without any structural

theory whatever, as early as the year 1830, by Hessel, who investigated in the most general manner all the possible modes in which a geometrical figure can be symmetrical; he then introduced a condition corresponding to the law of rational indices (*i.e.*, limited his conclusions to crystals), and deduced thirty-two types of symmetry.

Hessel's investigation was entirely overlooked for sixty years, and the problem was taken up anew by subsequent authors.

Gadolin, in 1867 (12), came to precisely the same conclusions; he began by introducing the law of rational indices, and found that all figures which obey this law, *i.e.*, all crystals, if grouped by their symmetry, fall into thirty-two classes.

The essential difference between their methods and that of the lattice consists in the fact that Hessel and Gadolin distinguish between different directions along the same line, whereas in the lattice no distinction is made between the two directions along the same thread of particles; to introduce this distinction something must be said about the shape of the particles; they must have a polar character.

Again, in the simple lattice parallel planes are absolutely identical, and a plane has the same aspect viewed from one side as from the other since the shape of the particles is not taken into account; hence such figures as the tetrahedron, which is not bounded by parallel planes, cannot be explained by the lattice.

Conclusions identical with those of Hessel and Gadolin have been independently obtained with different methods by subsequent investigators. All Hessel's varieties of symmetry belonging to geometrical figures in general have been recently found by Fedorow and Curie; while Gadolin's thirty-two crystallographic types have been established by Minnigerode and Schönflies.

The possible existence of these thirty-two types may now be regarded as a rigidly demonstrated geometrical fact, which must lie at the base of any views on the structure of crystals. Crystals belonging to all the types have

not yet been found in nature, but those of which examples are wanting are rapidly diminishing in number.

Only during the last few months the observations of Rinne (13) on scolecite, of Traube (14) on the antimonio-tartrates of barium, strontium and lead, and on the double salt antimonio-tartrate of lead with potassium nitrate, and of Penfield on caesium mercuric iodide (15), have contributed examples of four groups which were previously missing, while Traube has also shown (16) that lactose does not, as appeared from the early measurements of Schabus, present a symmetry different from all of the thirty-two types.

We may confidently expect that instances will shortly be found of the four yet missing types which are predicted by theory.

Let us now review the position to which our survey of the structural theories has brought us.

It appears from the researches of Bravais that a lattice structure accounts fully for the particular shape of crystals (*i.e.*, their subjection to the law of rational indices); from the researches of Hessel and Gadolin, that crystals so defined must belong to one or other of thirty-two symmetrical types; and, finally, from the researches of Sohncke, that the lattice structure is only a special case of a homogeneous assemblage of points, and that any such assemblage really implies a compound lattice structure; but that we cannot get the thirty-two types out of a homogeneous assemblage without assuming that the particles are of different kinds.

So far, then, two courses are open: we may either assume the law of rational indices as an empirical fact, and deduce from it the thirty-two groups; or we may start with a structure theory, in which the constituent particles are of different kinds.

In either case it is impossible to establish fully the varieties of form and symmetry of crystals by geometrical reasoning alone from the simple principle of homogeneity; and yet it seems remarkable that Sohncke's theory only just fails to do so; is it not possible that, after all, something may be lacking in Sohncke's treatment which deprives his method of perfect generality, and that all the thirty-two

types may yet be deduced from one single geometrical assumption?

The answer to this suggestion is supplied by another theory of crystal structure, which has meanwhile entered the field in a somewhat striking manner.

THE PURE STRUCTURAL THEORY.

Independently and almost simultaneously important investigations have been published by Fedorow (17) and Schönflies (18) upon the possible structures of geometrical figures and their symmetry.

Fedorow's researches were in reality published before those of Schönflies, but being in the Russian language, attracted no attention among mathematicians. The two authors have come to one and the same conclusion; namely, that there are 230 possible structures for crystals, and that these, if classed by their symmetry, fall into the now well-known thirty-two crystal groups. This absolute coincidence of results proves, if proof were needed, that the problem has been correctly solved; but before taking it for granted that the problem has been correctly stated, we must see what these authors mean by a crystalline structure, and in what manner their definition differs from that of others.

Schönflies defines a crystal as consisting of "absolutely similar molecules, so arranged that each molecule is environed in the same way by all the other molecules".

Sohncke's definition of a system of points which is to have the characters required by a crystal structure is that "round every point the arrangement of the remainder is the same as round every other point".

At first sight these definitions appear to be identical. How is it, then, that Schönflies arrives at 230 possible structures, while Sohncke finds only sixty-five? The difference, it will be found, lies in their ways of defining a similar environment.

Without entering into details, we may in a few words contrast the two methods.

Sohncke takes any point P of his system and joins it to

any number of adjacent points ABC, etc.; then, if we take any other point P′ of the system, it must be surrounded by points A′B′C′, etc., in such a manner that the lines PA, PB, PC, etc., can be exactly superposed upon the lines P′A′, P′B′, P′C′, etc. Hence he derives all his structures by considering in how many ways it is possible to move P to P′, and so on, by sliding, rotating and screw motions.

Schönflies does not assume that the lines PA, PB, PC, etc., can necessarily be only superposed upon P′A′, P′B′, P′C′, etc., but that PA = P′A′, PB = P′B′, PC = P′C′, etc., and that PA, PB, PC, etc., make the same angles with one another, as P′A′, P′B′, P′C′, etc.

Now this does not exclude the possibility of one set of lines bearing the same relation to the other that an object does to its image in a mirror; the two sets may be, in a sense, identical, though not superposable or congruent. Thus, for example, a point in the centre of a right hand glove has precisely the same environment as a point in the centre of a left hand glove, yet the two are not superposable; the one is, as it were, the reflection of the other.

In this way, then, Schönflies and Fedorow admit the possibility of deriving one part of their system from another by reflection; in Sohncke's method such a reflection may and does take place, but only as a result of the other movements.

Now the interesting feature of the new theory is that it is a purely structural theory; nothing is said about the nature of the particles; they may be identical throughout; and yet it accounts for the two essential features of a crystal, the subjection of the faces to the law of rational indices, and the existence of thirty-two types of symmetry.

The law of rational indices is more or less implied in the fact that all the 230 structures consist of interpenetrating lattices (just as in Sohncke's theory), and, as has been pointed out above, the planes of lattices obey this peculiar law.

In Sohncke's theory the law of rational indices is equally implied for the same reason; but the thirty-two types of symmetry cannot be deduced without assuming that the particles are of two or more different sorts.

The newer theory shows, for the first time, that these two essential features can be deduced by purely geometrical reasoning from a purely geometrical definition ; the arbitrary assumptions which mar the theories of Bravais and Sohncke are no longer necessary. It cannot be denied that this is an extremely interesting result, whether the theory have any physical meaning or no.

The new theory is, of course, more complicated than Sohncke's system, just as the latter was more complicated than the early theories of Wollaston and Hauy, or than that of Bravais ; there are, for example, no fewer than thirty different structures possible, according to Schönflies and Fedorow, for a holohedral orthorhombic crystal, as compared with nine in Sohncke's theory, and four in that of Bravais ; much more research is naturally required before we can determine which of these thirty is the most probable for a given orthorhombic crystal, such as barium sulphate.

On the other hand, it must not be forgotten that certain groups, such as the anorthic, and one mode of tetartohedrism in the hexagonal system, have only one corresponding molecular structure, even in Schönflies' theory ; it is in these that the structure theories are to be most easily tested.

A great advance has been made, if we can dismiss from our structures all molecules of a definite form, like the polyhedral molecules of Hauy, the ellipsoidal molecules of Wollaston and Dana, the pear-shaped molecules of Kekulé, and the other fantastic devices which have been introduced to account for crystal symmetry. It is also an advantage to escape the necessity of assuming any difference in the nature of the structural emits of a crystal; there is no direct evidence of any such difference. It remains to be seen how far the physical and chemical characters demand that such a difference should be recognised. Barlow and Sohncke have insisted upon the necessity of doing so, and Lord Kelvin introduces something of the sort into the structure which he has devised to account for the piezo-electricity of quartz.

It must be pointed out, however, that even a purely structural theory like that of Schönflies, although it supposes

all the particles to be identical, does implicitly recognise a difference in their *symmetry*. Two molecules with their respective environments which bear the relation to one another of an object to its image, must differ completely in their inherent symmetry, just as a right hand differs from a left hand. For this reason Sohncke (19) accuses Schönflies of constructing his crystal from two sorts of molecules which bear this peculiar and arbitrary relation to each other; right- and left-handed molecules; some structures consisting of right-handed, some of left-handed molecules, and some of both. We may remark, however, that the relation is not arbitrary, but results from the elementary principles of symmetry of position; further, that it harmonises very well with the characters of optically active substances which crystallise in right- or left-handed forms (dextro-chiral and laevo-chiral, as they are called by Lord Kelvin); these appear to be really constructed of right- and left-handed particles, for the character persists in the solution.

Here, however, we are beginning to travel outside the pure geometry, and to raise questions belonging to the physical aspect of the subject, which it is not the purpose of the present article to pursue.

Two remarks in conclusion. We have seen that the thirty-two types can be deduced from two assumptions, as was done by Hessel and Gadolin; firstly, that the symmetry of a crystal is homogeneous, *i.e.*, that in any one crystal a direction is symmetrically repeated in the same manner, *from whatever point that direction be taken;* secondly, that the faces of a crystal obey the law of rational indices. Now the first of these is a structural law, and is always true, in a physical sense at least; but the second is a law relating to the external form of the crystal, and is a question of superficial equilibrium between the crystal substance and the medium from which it crystallises; when a crystal is immersed in a solution which is not concentrated, it assumes a more or less rounded form, to which the law of rational indices has no application. No doubt the law is a result of the homogeneous structure, *under certain conditions.*

If we are to deduce the thirty-two groups from the simple principle of homogeneity alone, without assuming this law, we have at present to adopt a structural theory. Now there is no direct evidence that the constituent particles of crystals are different in their nature; *we are, therefore, driven to the theory of Schönflies and Fedorow.*

That theirs are purely geometrical structures in which the particles are mathematical points may be a recommendation rather than an objection to the theory, for it serves to remind us that the structure is to be regarded as merely a model or representation of something of which we have no nearer knowledge, and does not necessarily indicate the relative positions of the molecules any more than the figures of stereo-chemistry necessarily indicate the relative positions of the atoms.

The second remark relates to a point which is often overlooked. The structure theories do not assume that a crystal is necessarily symmetrical; they acknowledge that a crystal may be devoid of symmetry (calcium hyposulphite and acid strontium tartrate are examples); they only assert that a crystal must belong to one of thirty-two types, of which one has no symmetry, but the remaining thirty-one are symmetrical in various degrees; similarly they indicate that if a crystal is bounded by plane faces there is good reason for believing that these should be the planes of a Bravais lattice, that is, should obey the law of rational indices. Why a crystal should be symmetrical, or why it should be under certain circumstances bounded by plane faces, they utterly fail to show.

One objection which may be urged against the structural theories may be considered in conclusion. It may be said that they are mere mathematical exercises which have no physical meaning.

In reality, however, as Schönflies is careful to point out (20), what the structural theory does is this: it divides space into fundamental cells, within each of which analogous portions of matter must be distributed in an analogous manner, leaving the chemist, physicist and crystallographer to decide what are the characters of these portions of matter,

or to operate as they please with atoms and molecules; it defines the limits within which physical theories of crystalline structure may have free play.

BIBLIOGRAPHY.

(1) L. SOHNCKE. *Zeits. f. Krystallographie*, 13, 229, 1887.
(2) L. WULFF. *Verhandlungen russ. min. Ges.*, 25, 341, 1889.
(3) W. MUTHMANN. *Zeits. f. Krystallographie*, 22, 497, 1892.
(4) A. E. TUTTON. *Proc. Chemical Soc.*, 133, 1894.
(5) S. L. PENFIELD. *Amer. Journ. Sci.*, 47, 204, 1894.
(6) A. BRAVAIS. *Études Cristallographiques*, 1866.
(7) H. A. MIERS. *Nature*, 39, 277, 1889.
(8) L. SOHNCKE. *Entwickelung einer Theorie der Krystallstruktur*, 1879.
(9) L. WULFF. *Zeits. f. Krystallographie*, 13, 503, 1887.
(10) W. BARLOW. *Chemical News*, 53, 3-16, 1886.
(11) L. SOHNCKE. *Zeits. f. Krystallographie*, 18, 487, 1890.
(12) A. GADOLIN. *Acta Soc. Scientif. Fennicæ*, 9, 1, 1871.
(13) F. RINNE. *Neues. Jahrb. f. Mineralogie*, ii., 51, 1894.
(14) H. TRAUBE. *Neues. Jahrb. f. Mineralogie*, i., 245, 1894. Beilage Band, 8, 269, 1893.
(15) S. L. PENFIELD. *Amer. Journ. Sci.*, 44, 311, 1892.
(16) H. TRAUBE. *Neues. Jahrb. f. Mineralogie*. Beilage Band, 7, 430, 1891.
(17) E. VON FEDOROW. *Zeits. f. Krystallographie*, 20, 25, 1891.
(18) A. SCHÖNFLIES. *Krystallsysteme und Krystallstruktur*, 1891. *Zeits. f. Physikalische Chemie*, 9, 158, 1892.
(19) L. SOHNCKE. *Zeits. f. Krystallographie*, 20, 445, 1892.
(20) A. SCHÖNFLIES. *Zeits. f. Physikalische Chemie*, 10, 517, 1892.

H. A. MIERS.

CRITICAL REMARKS ON THE THEORIES OF EPIGENESIS AND EVOLUTION.

MR. G. C. BOURNE'S thoughtful paper in the April number of "SCIENCE PROGRESS" showed, in a very interesting manner, how in the most recent and popular evolutionary views—notably in those of Professor Weismann—we have an unintentional return to hypotheses of the eighteenth century (of Haller and Bonnet), which long ago were considered as definitely refuted and had been discarded accordingly. He also urged that this return was so far in conflict with the doctrine of epigenesis that the truth of the latter ran a risk of being thereby obscured and lost sight of.

Nevertheless there is evolution and evolution, and though the form of evolution he combats is open to his criticism, it does not follow that there is any conflict between epigenesis and evolution otherwise understood Indeed the idea of evolution, as I understand it, far from being antagonistic, is complementary, to that of epigenesis.

For me the term "evolution" has a predominantly dynamic implication, while, by the word "epigenesis," I would mainly refer to the material results of evolution—the facts concerning order, form and relation of a developing organism's several parts.

A process of epigenesis may be conceived of as taking place in two different ways, either mainly through external agencies or through an internal cause. Now for the development of the ovum a fit environment is of course necessary, as due physical external agencies are required for the development of a grain of corn. Still it is the nature and internal condition of the living corn-grain which is, *par excellence*, the cause of its growth, and such is also the case as regards the living ovum. The term "evolution" may be employed, as it has been, to denote that the successive formation of parts not previously existent is due, not to their imposition from without, but to their generation from within. Used in this sense "evolution" is not obnoxious to

the charge of implying the existence of minute parts each of which already possesses the essential characters of the body which are to result from their agency.

As to such hypothetical, material particles, Mr. Bourne has justly urged that when we try and get, by the aid of the imagination, behind and beyond what is perceptible to the senses, we become the victims of mere mental images, which he, after Wolff, terms *fabulæ*.

Hypothetical "gemmules," "plastidules," "micellæ," "inotagmata," "idiosomes," "plasms," etc. (however practically useful as working hypotheses), each and all, he tells us, when carefully considered, will be found no less to need explanation themselves than do the phenomena they are called in to explain. Such is the case since the "difficulty as regards vital phenomena" is not "in any way diminished" by their aid, but "simply moved further back". As to Professor Weismann's "biophors," he remarks,[1] they exhibit the primary vital forces, assimilation and metabolism, growth and multiplication by "fission," while "they are also bearers of the qualities of cells" and have "heritable qualities" which they can hand on to their progeny.[2] Again, he adds:[3] "Whitman must have his idiosomes to explain what he found inexplicable in cells, but what explains the behaviour of idiosomes?" Yet Mr. Bourne himself shows a favour to "molecules," which he denies to imaginary entities of supposed larger dimensions. He tells us:[4] "We may concede a great complexity of molecular structure to the idioplasm of the nucleus". Of course we may; but what then?

[1] p. 113.

[2] According to Weismann each biophor is also capable of changes and modifications by means of rearrangements of its constituent molecules. Now such changes and modifications cannot be uncaused but must be induced in the biophor during its life by its environment. But the characters of the biophor are *heritable*. Therefore, the biophor is a living creature, which is capable of transmitting acquired characters to its posterity. This is surely no small *argumentum ad hominem* in favour of their transmission by organisms, of which the biophor is but a diminutive representative!

[3] p. 124. [4] p. 125.

Yet, in truth, neither Mr. Bourne, Professor Whitman, Professor Nägeli, Professor Weismann nor any other inventors of such mental images are to be blamed, or in the least disesteemed for their failure in presenting satisfactory explanations. Such failure is but the inevitable result of attempting the impossible. However we may minimise or subdivide such supposed material elements, the same difficulty will ever recur. In the same way as when we seek to explain the physiology of an organism by the functions of its "cells," each cell so considered becomes but an organism itself "writ small"; so every biophor, idiosome, etc., of a cell again becomes the cell,[1] again "writ small". This is the case with the "molecules" also, since the mere juxtaposition of functionless, similar particles can never account for vital phenomena, such as growth and reproduction, to say nothing of sensation and reflex consciousness! However we may play with such images by the aid of a subtle and fertile imagination the same inevitable and insoluble difficulty will ever recur. Indeed the most fertile producer of imaginary hypotheses of our own day, has really made this confession by declaring that so long as we know practically nothing about the *forces* which act in and among biophors, they cannot afford us what he regards as an explanation of vital phenomena.

We can decline to seek for any explanation and then we may rest contented with the formula of epigenesis, which sums up the facts which we can perceive by the aid of our senses. But if we seek, as our nature impels us to seek, a cause which may explain the facts of epigenesis we shall be forced to have recourse to one of a radically different nature. Mr. Bourne does not seem to see this for he says:[2]

[1] Mr. Bourne softens down somewhat too much the difference which has been commonly thought to exist between unicellular and multicellular organisms. I welcome, however, the minimising of that difference, for I cannot regard the contractile fibre of *Vorticella* as something essentially different from a muscular fibre, for the sole reason that the former is the product of a single cell. The real equivalent in a man of the single cell of *Vorticella* is, for me, nothing less than the man's whole body.

[2] p. 113.

"The arguments of those who plead for the existence of infinitesimal units, whether micellæ or pangenes or biophors or what not, amount to this: so far we have been able to observe with our senses, but the things which we have observed remain unintelligible to us; reason tells us that behind all this there must be something which we are at present unable to observe, and analogy leads us to believe that this is in the form of material particles".

Such conceptions are necessarily unsatisfactory for the following reason: In the various activities commonly called "vital," we can perceive various parts of an organism under successively different conditions; but the activities themselves, whatever may be their nature, are not also material bodies and are utterly imperceptible to our senses. Now we can never imagine anything of which we have had no sort of sensuous experience. Biophors, micellæ, inotagmata, pangenes and molecules, are terms for mental images of material particles which differ only from bodies perceptible to the senses, because they are supposed to be of exceedingly minute size. They are, therefore, necessarily incapable of making us understand anything immaterial, and the use of them amounts to an attempt to make imaginary representations of things perceptible to the senses serve as representations of things imperceptible to the senses and therefore essentially incapable of any such representation.

The irrationality of this is very often unnoticed, because the imagination of an immense multiplication and complication of minute parts and their motions, tends so to fatigue the fancy as to make some persons think that by having had their imagination thus overwhelmed by a complication of images exceeding its grasp, they have arrived at something of a really different nature and capable of explaining phenomena the senses can take cognisance of.

Can we attain to any more satisfactory explanation, or must we sit down contented, or discontented, with the assertion of epigenesis as a fact?

We have seen Mr. Bourne himself affirm that reason declares the existence of something else behind what we can observe.

Now, in every investigation our first duty is to inquire what facts are most certain and evident, and what consequences evidently flow therefrom, and our various speculations and hypotheses must be judged by the results of such inquiry.

In studying the activities of organisms, therefore, we should first make use of that about which we can obtain the most evident and certain knowledge. But of all organisms no one is nearly so well known to us as is our own. Nothing then can well be more irrational than for us to ignore our self-knowledge, while seeking to understand the nature of other living organisms.

Prominent amongst the "humble philosophy of facts" is the fact that we not only think but can know we are thinking, that we can know much of our own past in addition to our present, and that we can consider and reconsider our thoughts in different groups and serial orders, passing them in review, as it were, before a present consciousness of them and of ourselves.

If we are certain (and I think only an unsound mind can doubt it) that we have—that we consist of—a material extended body, it is yet more evident and indisputable that we have—that we consist of—an individual immaterial energy. Of much which this energy can accomplish we are directly conscious. That the phenomena of the body are influenced thereby is certain, while our conscious psychical activity shades off into activities of which we are entirely unconscious.

On the principle of parsimony we cannot, in the absence of evidence, deny that this immaterial psychical energy (which shows itself in our conscious mental activity and acts in states of our being, ordinarily unnoted, but which can be known to consciousness), also energises in all the body's unconscious activities.

I do not, of course, mean we are conscious of anything within our body and distinct from it, but that we are conscious of being both a material organism and an immaterial energy, and that the latter, during healthy life, is dominant and directive. This indeed is shown by every act we consciously perform in obedience to our will.

Here is one absolutely certain and evident fact, of which every biologist is bound to take account as being the most certain of all biological facts.

Since, then, we know this fundamental verity with respect to the animal man, it is reasonable to suppose that other organisms which resemble man in various degrees each of them also consists of an extended material structure and an individual immaterial energy, however different it may be from that of man in its powers and faculties.

It is now becoming more and more distinctly recognised that each organism in the course of its development and its subsequent life-history is dominated and governed by some individuating agency. Professor Burdon Sanderson's words at the penultimate meeting of the British Association, though avoiding any direct consideration of the problem, decidedly favoured such a view. As we have seen, Professor Weismann recognises the existence of a, to him inscrutable, force as acting in every biophor, and Professor Whitman has declared "that organisation precedes cell-formation and regulates it rather than the reverse, is a conclusion that forces itself upon us from many sides".

That a living organism is a unity consisting of an immaterial individuating energy, immanent in a variously differentiated mass of matter which it dominates and regulates, is a conception which the imagination cannot picture. That circumstance, however, is no bar to its satisfactory character for the intellect, because it harmonises and accounts for the phenomena of organic life in a way which accords with the most certain of all our knowledge about our own organism.

Mr. Bourne speaks[1] of "unmeaning phrases, such as formative forces, vital principles and the like". I echo his words in so far as no "vital force" or "principle," apart from real individual organisms, is for me either a perception or an inference. Nevertheless, the existence of our individuating, immaterial, psychical power is a matter of direct perception and therefore as full of meaning as can well be, while as to other organisms the existence of a parallel power

[1] p. 113.

is a matter of reasonable inference ; as directly depending on the highest possible evidence it is emphatically scientific. This conception was not "unmeaning" to one of the greatest of human intellects, for it was that of Aristotle who taught it. It is practically accepted, indeed both by Weismann and Häckel. The former, as before said, credits biophors with peculiar forces, while the latter is ready enough to attribute an individuating energy to every cell. Why then are such forces to be denied to individual animals and plants ?

But Mr. Bourne appears to adopt a mistaken view put forward by the late Professor Tyndall. The latter regarded an ability to "mentally visualise" a conception as a necessary condition of its truth. Yet to "mentally visualise" anything immaterial would be a contradiction, and a "mentally visualised" notion of the kind must, by the mere fact that it is so visualised, be a false notion.

Students of science are often too ready to bow down to authority and accept the dicta of men of wide repute without examining what such dicta may be worth. They thus become the victims of prejudice, forgetting that popular opinion, even scientific popular opinion, is of no avail against facts and logical deductions therefrom.

It is time to protest vigorously against the statement that the several departments of science should each be kept "carefully apart" from the highest department of science—from that which alone gives validity to every other. Such a statement is really as absurd as it would be to say that arithmetic should be kept carefully apart from the philosophic conception of "number," and geometry from that of "extension".

The impossibility of teaching physical science in terms of mere phenomena has seldom been more strikingly exemplified than in Professor Pearson's recent work on physics—an attempt to effect the impossible. In the conception here advocated as the one only satisfactory explanation of organic life, we have what no one can deny to be a *vera causa*, one which is supremely evident in and to ourselves and which we may, by the most rational analogy, extend to other

organisms. While satisfying the mind as being an intelligible, though unimaginable cause, it tends neither to stifle nor to limit inquiry. Nothing here advanced tends to depreciate the cell-theory, nor any other physical hypothesis of practical utility. Let the essence of each living creature be an active, immaterial principle of individuation, it remains not a whit less true that the various cells of the tissues are conditions *sine quâ non* of growth and that "the whole organism subsists only by means of the reciprocal action of its single elementary parts".

This conception being accepted a practically inexhaustible field of research will none the less ever remain open for the discovery of the successive hierarchies of material agents, which intervene between the immanent prime cause of each organism's activity and the ultimate manifestations of the latter. The structure of the cell, its nucleus, its more minute structures and the interactions of the many parts which yet remain to be discovered, are in no way less interesting to one who accepts the view here advocated than to him who is unable or unwilling to rise above the imagination of material particles exclusively.

St. George Mivart.

ELECTROMOTIVE PHENOMENA IN GLANDS.

IN so many instances of physiological action has it been demonstrated that there is a development of electromotive force, that the physiologist almost expects to find an electrical expression of any essential vital phenomenon under his consideration.

In muscle and nerve, the story has been studied now for upwards of a century, and the mass of our knowledge of the processes underlying the development of electricity in animal tissues, culled from these, the oldest sources, forms the base upon which the theory of the subject mainly rests, the standard to which the points of more recent discoveries are frequently referred.

What knowledge we have of the electrical events taking place in secreting cells is, however, of fairly recent date, and although the accumulation of facts is considerable, their interpretation is as yet a matter of great difficulty.

The glands of the frog's skin have all along been favourite structures for the investigation of the matter, on account of the great ease with which a nerve-skin preparation is obtained, and it will be convenient to first sketch the facts known regarding these organs.

Emil du Bois-Reymond, in 1851, in his endeavours to demonstrate the existence of electrical currents in the muscles of uninjured frogs, discovered that the skin of the animal was itself a source of electromotive force, which was altogether of too high magnitude to be attributable to mere differences in chemical reaction between the parts to which the electrodes were applied. Finding that removal of the surface layers containing the glands abolished the current, and that the skins of fish, which he considered non-glandular, gave no such current, he concluded that the glands of the skin were a source of electromotive force with ingoing current.[1]

[1] Throughout this article, the terms *ingoing* and *outgoing* applied to gland currents have the following significance: Ingoing current means a

Rosenthal soon corroborated the truth of the original observation, and likewise considered that a "gland current" was under observation, since he found the source of electromotive force lay nearer the outer than the inner surface of the skin; he further demonstrated that a similar ingoing current could be derived from both the acid stomach and the alkaline gut in both frog and rabbit.

A "current of rest" of probable glandular origin having been shown to exist, it was necessary to obtain evidence of "current of action" as result of excitation of the nerves of the skin glands.

The first systematic research upon the subject was by Roeber, under Rosenthal's direction, though it should be mentioned that Valentin had seen the effect several years previous.

Roeber observed an outgoing "current of action" (*i.e.*, negative variation of the current of rest) to be the rule, though the matter was complicated by the occasional appearance of an ingoing current. The effect was not affected by curare; indeed, his most striking experiment was with a curarised and subsequently strychnised frog, in which a tap on the table sent the galvanometer needle into "Zuckungen," while the frog lay motionless.

The matter shortly became the subject of a masterly research by Engelmann. Having made an exhaustive microscopic examination of the glands of the skin and *membrana nictitans* in the living state, paying especial attention to the conditions of the contractile phenomena first described by Ascherson, Engelmann turned his attention to the electrical changes. Imbued at the time with the "Präexistenz Theorie" of Du Bois-Reymond, for Engelmann the current of the "resting" skin derived its electromotive force from the fibres of the contractile sheath of the glands, and this electromotive force he considered as the "Trieb-

current running *in the gland* from neck to base, *i.e.*, neck galvanometrically negative to base; so also in a unicellular gland (goblet cell) current from superficial to deep end of the cell. Outgoing has, of course, the opposite meaning. The adoption of these expressions saves to a large extent the confusion liable to arise from the use of the terms "negative or positive variation".

kraft" for the passage of fluid across the living cells in an act of secretion.

Says Engelmann: "Die Absonderung der Hautdrüsen ist nichts Anderes als electrische Osmose".

The "negative variation" of the "current of rest" of the skin, upon excitation of its nerves, he considered as due to the "negative variation" of the pre-existing current of the contractile cells. Thus the forcing of fluid into the lumen of the gland was considered as taking place continuously, while at intervals, when the outer sheath contracted, expulsion of the contents of the lumen took place, and for the time being the "Triebkraft" was diminished. Engelmann, indeed, always obtained the highest readings for the electromotive force of the "current of rest," under conditions which he knew from microscopic work to be favourable to dilated glands with resting "muscle" sheath. Later, direct observations upon the electromotive force of smooth muscle fibres from the stomach of the frog seem to have raised doubts in Engelmann's mind as to the validity of his original "myogenic hypothesis," and the idea is noted here rather for its ingenuity and historical interest than for any other purpose. The actual observations are, however, in many respects of great value, and it should be noted here that Engelmann observed something of the nature of a "double current" (outgoing followed by ingoing) upon excitation, though he considered the second effect as of the nature of an "after effect".

Hermann next studied the subject. In his first paper he does not commit himself to any definite explanation of the source of electromotive force of the "resting" skin, though on the idea that some cell processes may underlie the matter, he hints at some difference of action in the mucous and granular varieties of skin glands being the cause of the diphasic variation, which he also observed. The normal direction of the "current of action" is, according to Hermann, ingoing, though there is often a preceding outgoing "Vorschlag". This statement is quite opposed to the finding of Engelmann, who, as a rule, got outgoing action current on stimulation.

Two pupils of Hermann, Bach and Oehler, continued the work, and made the important observation that the direction of the action current could be varied at will by certain methods of treatment, *viz.*, heating to 50° c., pencilling the skin with saturated mercuric or sodic chloride solutions, and by repeated excitation. By such procedure, an action current previously outgoing in direction could be made ingoing, though naturally by such treatment a fall in the electromotive force of the "current of rest" was to be expected, and, indeed, was observed.

Bach and Oehler also found that atropine abolished the excitatory variation, making it all the more probable that such variation is of glandular origin.

By the treatment with mercuric chloride, it was found that while the "current of rest" could often be completely annihilated, a "current of action" could still be elicited, and this fact, together with his observation, that the skin of the fish (which he considered "non-glandular") gave an ingoing "current of rest," led Hermann in a second paper to formulate a distinct theory of the electromotive action of the frog's skin.

The electromotive force of the "current of rest" (since he found such a current in the so-called "non-glandular" fish skin) Hermann considered as epidermic in origin, and on the Alteration theory to be produced by the keratinisation of the surface of the epidermis acting as a demarcation surface in protoplasmic continuity. No component of the current of the spheroidal skin glands with closed ducts is considered as derivable at the surface touched by the electrodes, so long as the glands are in the resting state.

On excitation, however, the contraction of the gland puts the interior into direct communication with the surface by the extruded thread of slime. Should the electromotive force of the gland cells be less than that of the epidermic cells at the moment that the "lead off" is established, a "negative variation" of the skin current must occur. When, however, the gland cells become active (and in the case just mentioned one is supposed to be within the latency of the secreting cells, though, of course, not within that of the

contractile sheath) their electromotive force exceeds that of the epidermis, with the effect of giving a positive variation of the "epidermic" current of rest. In this way Hermann explains the diphasic variation most frequently observed, *viz.*, a short outgoing current (negative Vorschlag), followed by an ingoing current, and is of definite opinion that while the electromotive force of the "action current" is glandular, that of the "rest current" is of epidermic origin.

Bayliss and Bradford, who subsequently looked into the matter, consider that the "rest current" of the frog's skin is partly epidermic and partly glandular, since corrosive sublimate reduces the "rest current" without cutting out the "action current," while subsequent treatment of the skin with atropine, while it cuts out the "action current," also still further reduces the "current of rest". Time of year is also considered by these observers as an important factor as regards the nature of the action current, outgoing currents predominating during and just before the breeding season, while during the rest of the year an ingoing phase is the rule.

Before passing to the most recent work upon the currents of the frog's skin glands, it will, perhaps, be convenient to briefly mention the other cases in which gland currents have been studied.

Hermann and Luchsinger, in 1878, discovered the presence of an ingoing "secretion current" in the sweat glands of the pad of the cat's foot, upon excitation of the peripheral sciatic, and this effect was abolished by atropine, and unaffected by curare. They make no mention of any outgoing phase, and as regards the current observed, state that it is quite distinct, even when no exudation of sweat is visible. The effect in the classical "Willkür-versuch" of Du Bois-Reymond is considered as a sweat gland secretion current on the ground of these results.[1]

[1] Tarchanoff has recently shown that parts of the surface of the skin of man, rich in sweat glands, are electrically negative to parts poor in such structures, and, further, that the ingoing "secretion current" can be developed under the most diverse conditions of disturbance of the central nervous system. Thus with leads of moistened cotton-wool from, say,

In the same year, the same observers investigated the "secretion currents" of the lingual glands of the frog. Here, again, an ingoing "current of rest" was observed, and upon excitation of either glosso-pharyngeal or hypoglossal nerves, but especially of the former, a triphasic variation appeared, the phases being in the order, ingoing, outgoing, with final ingoing. This effect was obviously due to the interpolation of an outgoing phase during the course of a long lasting ingoing current.

Luchsinger also, two years later, experimented with the glands of the snout of the pig, goat, cat and dog, and while he found the "rest current" ingoing, could only elicit an ingoing "current of action" on excitation of the cervical sympathetic or the infra-orbital nerves. Difference of reaction of the electrodes made no difference to the direction of the "rest current," and the glandular nature of the "action current" was again indicated by its abolition by atropine.

The cases, then, that have so far been considered, show that it is the rule for the "current of rest" of a gland to be ingoing in direction, while the "current of action" appears to be in the same direction in the majority of cases, though there are cases and conditions in which the galvanometer shows an opposite (outgoing) current, or a di- or even tri-phasic variation, composed of two opposed currents of unequal strength and duration.

It is to the merit of Bayliss and Bradford to have been the first to point out clearly the fact that it is probably always the rule for the electrical changes in secretion to be diphasic, and, moreover, that the separate phases may be associated with differences of physiological action in the gland cells, related possibly even to specific sets of nerve fibres.

These observers made extended investigations into the

palm of hand, and skin over deltoid muscle, such diverse stimuli as tickling the face, smelling acetic acid, a sound, or even the expectation thereof, mental arithmetic, etc., may cause a marked current indicative of relatively richly glandular palm skin, negative to poorly glandular shoulder skin. Withdrawal from other disturbances than those needed for the experiment is necessary for success with this experiment.

electrical phenomena of the salivary glands of the dog and cat, and concluded that while the variation upon excitation is diphasic in all cases (with, perhaps, the exception of the parotid of the dog on sympathetic excitation), the phase in the ascendant is determined by the predominance of one of the two factors in the hypothesis of Heidenhain in the cell action, *viz.*, flow of fluid, and elaboration of organic constituents.

Flow of fluid they hold is marked by outgoing, cell metabolism by ingoing current. Thus excitation of the chorda of the dog's submaxillary gives a predominant outgoing phase easily cut out by atropine, while excitation of the sympathetic is marked by ingoing current, and the variation is refractory to atropine. In the case of the cat, as a rule, the "chorda saliva" is more viscid than that from sympathetic excitation, and in the latter case the outgoing current is more prominent, while though both phases are distinct with the chorda on the electrodes, the ingoing phase is far more marked than is the case with the dog. In the exceptional cases, where the "sympathetic saliva" of the cat's submaxillary gland is very viscid, a marked ingoing current makes itself evident.

It is possible even for a galvanometer reading to indicate an outgoing current, pure and simple (in, say, the dog's submaxillary, on excitation of the chorda), though abolition or reduction of the outgoing phase by atropine may reveal the existence of a contemporaneous ingoing current that had previously been swamped.

The net conclusion arrived at by these observers is that the outgoing secretion current, so marked in free flows of saliva, is electro-capillary in origin, whether produced in the basement membrane or the cells, while the ingoing secretion current, appertaining more especially to cases of viscid secretion, is produced during the metabolic cell events necessitated in the elaboration of the organic constituents of the saliva. The ordinary galvanometer reading is naturally the algebraic sum of the two opposing currents.[1]

[1] Langley has adopted this view with the difference that he maintains as the result of his experiments with atropine, that the explanation of the

There appeared last year a paper by Biedermann in which the electrical phenomena in several cases of mucous glands are detailed, and the results are apparently so consonant in all the structures investigated, that the work not only carries considerable weight, but the theory of action deduced by the author must certainly rank as one of the best put forward, especially as it tends, in one case, that of the frog's skin, to reconcile some of the opposing results obtained by Roeber and Engelmann on the one hand and Hermann on the other.

The experiments deal with mucous glands, either multicellular as in the tongue and skin of the frog, or unicellular (goblet cells) as in the pharyngeal and cloacal mucosæ of the same animal.

Bohlen under Biedermann's direction has continued the work in the case of the gastric mucosæ of the frog and certain mammals.

It is convenient and conducive to brevity to first sketch Biedermann's theory before stating the facts upon which it is built.

Biedermann, adopting Hering's extension of the alteration theory, *viz.*, that alterations in metabolism rather than of chemical composition in protoplasmic continuity, lie at the root of vital electrical changes, points out that in the gland cell we have a structure pre-eminently liable to variations from the balance between constructive and destructive events. That part of the protoplasm of the cell engaged in the elaboration of the secretory products, though ordinarily the seat of an excess of katabolic over anabolic activity, must at the same time be in a highly unstable state and liable under certain conditions to an overthrow of balance to the opposite side.

action of the drug is not that the flow of fluid in the cells is completely stayed, so that the whole variation is produced by the " metabolic events," but rather that, since according to his results all cell activities are equally acted upon by atropine, in cases of slight cell action, in highly poisoned but not paralysed states, the flow of fluid is proportionately more cut down than the series of metabolic events, which Bayliss and Bradford consider are the cause of the ingoing current.

The electrical sign of a part in which katabolism is in excess of anabolism ("absteigende Aenderung" of Hering) is negative as contrasted with that of a "balanced part," more negative still to a part where anabolism is in excess of katabolism ("aufsteigende Aenderung" of Hering) and of course *vice versâ* as regards positivity.

Thus, it is supposed, differences of potential may be originated in the cell as the result of its vital processes, and the cell current will depend, as regards its direction, upon the predominance of either one or the other process and the relation of the resultant condition to that of the rest of the protoplasm.

The act of production of the secretion by the cell is one in which katabolic action is in the ascendant, so that that part of the cell in which the secretion is occurring should be negative to the deeper parts with a resultant ingoing current when put in circuit, and upon this hypothesis Biedermann explains the ingoing "current of rest" as being the electrical expression of a continuous and predominantly katabolic activity. Should it be possible to overthrow the balance to the anabolic side a reversal of "rest current" ought to occur, and such a condition is known to be capable of production.

Biedermann then is of opinion "dass eine und dieselbe Epithelzelle und zwar in fast gleichem Grade bald in dem einem und bald in dem anderen Sinne elektromotorisch zu wirken vermag," in fact "dass jede Zelle als Sitz von 2 verschiedenen chemischen Processen auszusehen ist, die gleichzeitig vorhanden, zur Entstehung gegensinniger Spannungen führen. Die jeweils zu beobachtende Ablenkung würde demgemäss immer nur die Resultirende aus 2 antagonistischen Kräften sein."

Of the two sets of cell processes, the katabolic (called "negative process" by Biedermann) is considered as less resistant to contra-normal conditions than the anabolic ("positive process").

As regards the ultimate nature of the processes, secretion of water is suggested as the katabolic process, since in cases in which a mucous surface is found bedewed with secretion the electromotive force of its "current of rest" is always

higher than when it is dry, and as the anabolic process—the building up of complex organic paraplasm.

A further point in this theory remains to be mentioned, namely, that the excitation of a gland cell tends towards the temporary increase, not of the process for the time being in the ascendant, but rather of the process in abeyance, so that if the activity is at the moment of excitation predominantly katabolic, the immediate result will be a higher development of the anabolic process and *vice versâ*.

The points gained by experiment upon which the above theory is based are as follows as regards the "Current of Rest":—

By abstraction of heat it is possible to completely reverse the normal "current of rest" from ingoing to outgoing, and consequently at a certain temperature it is possible to have a currentless gland, the raising of the temperature of which produces an ingoing, lowering an outgoing current. As is well known no such phenomenon is observable with the "demarcation current" of muscle.

Imbibition of water by the cells leads to increase, extraction of water to decrease or reversal of the "current of rest".

Narcotics, as carbonic acid or vapour of ether or chloroform, also reduce or reverse the "current of rest".

Pilocarpine and often continued moderate nerve excitation tend to increase the "current of rest".

The "current of rest" undergoes marked "spontaneous" variations (a fact which has been noted by many previous observers).

As regards the "Current of Action":—

The direction of the "current of action" is dependent upon two factors, the extent to which the normal ingoing "current of rest" is developed, and the strength of the stimulus. With well-developed normal ingoing "current of rest," and especially when the stimulus is strong, the rule is outgoing "action current". With ill-developed normal "rest current," and weak stimulus, the rule is ingoing "action current". Thus by artificially reducing the "current of rest" by water abstraction, it may be possible

with one and the same stimulus, applied at intervals during the removal of water, to start with an outgoing "action current," and passing through diphasic variations with a predominant phase in either direction, to finally arrive at a stage with purely ingoing "action current," when the "rest current" is reduced to the utmost. In cases of artificial reversal of the "current of rest," so that it becomes outgoing, the "current of action" becomes ingoing.

These experiments, especially those with the non-muscular tongue glands of the frog, overthrow the hypothesis of Hermann above mentioned, not only by the demonstration that contractility is not a necessary factor in the phenomena, but by indicating in the phenomenon of "reversal" that the rather crude hypothesis, that the "current of rest" owes its source to a keratinised or mucous demarcation surface, is not tenable. Indeed, previously published experiments with the secreting skin of the eel had already demonstrated the difficulty of Hermann's position.

The effect of Bach and Oehler in converting an outgoing into an ingoing "action current" in the frog's skin, by pencilling with corrosive sublimate, is explained by Biedermann as being due to the reduction of the "current of rest" produced, and the dependence of the direction of the "action current" upon the magnitude of the "rest current".

Bohlen, in his experiments with the gastric mucosæ, corroborates the main points of Biedermann's theory. In the case of the frog, he considers that the ingoing "current of rest" of Rosenthal is in the main, if not entirely, the result of the activity of the superficial mucous cells, and is not contributed to, to any large extent, by the gastric glands. Ordinary digestive activity (feeding with meat) does not markedly affect the current of rest, while the introduction into the stomach of substances tending to produce excessive secretion of mucus, such as small sharp pebbles, hard scaled insects, or the minute angular crystals of subnitrate of bismuth, causes a great increase in the electromotive force of the "current of rest" of the stomach. Vagus excitation in the frog produces an ingoing "action current," and this he thinks is possibly of gastric gland origin, since

excitation of mucous cells is generally accompanied by outgoing "action current".

In the mammals (rabbit, guinea-pig and rat), nothing definite was found as regards the action of digestive activity, except that the "current of rest" tends to fall during the earlier stages of digestion, and to rise towards the end. Interference with the blood supply of the mucosa was found to be a potent factor as regards the electromotive force of the "current of rest". Complete cessation of circulation causes speedy reversal of "rest current," while more moderate interferences, such as that produced by dyspnœic or anæmic excitation of the bulbar vasomotor centre after double vagotomy, or by excitation of the peripheral vagus, all produce a drop in the electromotive force of the ingoing current. Atropine, probably by cutting out the cardiac vagus effect, prevents the fall of E.M.F. that results from vagus excitation. Saline injections into the vascular system, accompanied by marked secretion and swelling of the mucosa, lead to a marked rise of the electromotive force of the "rest current," and such current does not then fall rapidly after death, as is normally the case.

Bohlen considers that his results in cutting down the ingoing current by diminution of blood supply, and in raising it by causing excessive exudation of water into the membrane, are in favour of Biedermann's idea, that the "negative process" in the cell is the secretion of water. Before passing to any further consideration of the respective theories of action already mentioned, a word must be said as regards what is probably the highest development of electromotive phenomena in glandular tissue. Fritsch has maintained that the electrical organ of the fish *Malapterurus* is constructed of a mass of glandular cells, known as "club cells," and derived originally from the overlying skin, in which in the adult many are to be found. The theory is extremely probable, but, unfortunately, as yet lacks embryological basis.

These "club cells" are present in the epidermis in other fish along with "goblet cells," and it has recently been pointed out in the case of the skin of the common eel, that

while the "action current" of a part rich in "goblet cells," and clear of "club cells," such as the lip, conforms in its conditional modifications to that described by Biedermann for ordinary mucous surfaces, the current from the body skin, which is richly set with "club cells," but sparsely so with "goblets," does not agree with Biedermann's story, but gives an ingoing "action current" to a stimulus that elicits an outgoing current in the mucous lip. The discharge of *Malapterurus* is peculiar in that the nervous sides of the electrical discs become positive to the non-nervous, and the observation as regards the direction of the action current of the club cells of the eel just mentioned, gives physiological backing to Fritsch's idea, and would serve to explain the anomalous direction of the "shock".

We have now briefly reviewed the main points of the electrical phenomena of glands, and it yet remains to attempt some criticism of the various theories advanced.

One point appears clear at the start, namely, that any theory of the "action current," if it is to be in any way satisfactory, must also be applicable for explanation of the "current of rest". A piece of exsected frog's skin or nictitating membrane exhibits marked variations of potential, often quite equal to anything that can be called forth upon excitation, and these spontaneous cell changes must be of the same kind as those excited by nerve stimulation. Again, artificially induced reduction of the "current of rest," or such as is noted in animals "out of condition," at once profoundly affects the result of excitation.

In the face of such facts, the demarcation surface theory proposed by Hermann to explain the "current of rest" cannot stand, at any rate in the form in which he put it forth; for if the ingoing "current of rest" is merely due to the chemical alteration that the surface cells of a glandular membrane have undergone during growth, how shall we explain the complete "reversal" and "recovery" that can be repeated experimentally any number of times in succession? That epithelial surfaces, which are not secretory in nature, can give ingoing currents of rest, has been demonstrated in the case of the non-glandular skin and crop of

the winter pigeon, but there is no keratinisation in the latter case, and in the former the change certainly does not favour the electromotive force. Again, though Hermann ascribes the electromotive force of the action current to gland activity, his theory does not explain matters fully till a contractile sheath is added to the gland; yet the tongue glands of the frog or the skin of an eel give good enough action currents without muscle.

The "myogenic theory" of Engelmann is also for the last reason invalid, let alone the fact that in cases where muscle is present it rests upon the rejected "pre-existing theory" of muscle current.

We must with Biedermann consider the normal "rest current" of a gland—and it *is* normal, and not due to "injury"—as the electrical expression of a continuous chemical activity in its cells, and that on excitation we deal, not with a new electromotive force originating from other sources, but simply with a change in the action of the same structures, which are the origin of electromotive force also in the condition called, by contrast, that of "rest".

As regards the admittedly diphasic nature of the "current of action," as we have seen, two theories may be entertained; that of Bayliss and Bradford, deduced from experiments with the mammalian salivary glands, and that of Biedermann, built upon facts obtained with mucus-secreting glands and membranes.

According to the former theory an outgoing "action current" is the result of flow of fluid in some part of the secreting mechanism (physical and indirect); ingoing current is associated with the elaboration by the protoplasm of secretion stuffs (physiological and direct).

According to the latter theory, both outgoing and ingoing "action currents" are the direct result of cell activity, *i.e.*, both physiological, but the two currents are paired with the two hypothetical sides of cell life, dissimilation and assimilation. If, however, we follow Biedermann into his further speculation as to the ultimate nature of the two sets of processes related to the cell currents we find he reaches the opposite conclusion to Bayliss and Bradford, and gives

water secretion as the process associated with the ingoing current, and the formation of complex paraplasm as that connected with the outgoing current.

One cannot doubt that water flow in cells and ducts must give current, for the phenomenon is well known in porous diaphragms, capillary tubes, and also in plants; but if we accept water flow as the *main cause* of the outgoing action current, how are we to explain the marked outgoing currents produced in the reversal of the current of the glands of the frog by abstraction of water (and be it noted abstraction from the basal ends of the cells) by injection of glycerine, when it has been granted that explanations of "action" and "rest currents" must stand or fall together?

The events in the protoplasm of the gland cell are as yet obscure, beyond the fact that it builds secretory substances, which dissolve in water to become the secretion. By what means the water crosses the cell from lymph to lumen and in so doing produces the "secretory pressure" is unknown. The "pump theory" of Stricker and Spina, elaborated from the well-known movements of the cells of the glands of the *membrana nictitans* of the frog, will not hold according to the more recent and exact work of Drasch, let alone the fact that we have no clear evidence of anything of the kind in the case of mammals.

The older theory of Hering of the formation of colloid substances by the protoplasm, their swelling by water abstraction from the lymph (in order to account for the secretory pressure), and their final extrusion into the lumen, is perhaps as much in accord with the histological story as any other. Granules do dissolve in cells, according to Langley and Biedermann, who have watched the process *intra vitam*, but the chemical solution of such granules is not a likely source of electromotive force, and certainly a streak of mucin granules from *Myxine*, connected at each end with a sensitive galvanometer, gives no constant result upon causing solution of the granules to proceed in the region of one or the other electrode.

Against the water flow theory, it must also be noted, that, so far as measurements go, the latency of the outgoing

action current is far shorter than that for the flow of fluid in the ducts even when a high pressure is established in these by artificial means. Bayliss and Bradford measured the former upon excitation of the dog's chorda at ·37 sec., while Hering measured the latter, distending the ducts at a pressure of 150 to 200 mm. of mercury and using a Fick's spring manometer, at the shortest as 1·2 secs.[1] If then we are forced to exclude electro-capillary current, as at any rate the principal factor in the phenomena, we are forced back upon the theory of Biedermann, and it must be admitted that, though the explanation is based upon the obscure "chemismus" of protoplasm, it has much in its favour.

It is, however, difficult to see clearly what right Biedermann and Bohlen have for considering water secretion as the cell process connected with the production of the ingoing current, for the fact that increment of the water in the cells swells the ingoing current might surely also receive explanation on the more general ground of increased protoplasmic activity, since it is generally accepted that there is a relation between the activity of a tissue and the amount of water it contains. In the skin of the eel water has a distinctly deleterious effect, reducing or reversing the normal current, though the effect is done away with by substituting normal saline solution.

Whether or no water secretion has any direct connection with gland currents, might perhaps be decided by using for experiment cases in which there is no secretion of water, as, for instance, the uropygial gland of the bird or the Harderian of the mammal, or perhaps the crop of the breeding pigeon, which in its lateral lobes becomes practically a sebaceous gland during the time the birds are giving "milk" to their young.

In conclusion, it may conduce to clearness to put in tabular form the explanations offered by the two most important theories for some of the ordinarily observed experimental facts.

[1] Ludwig originally measured this latency with a mercurial manometer at from four to twenty-four seconds.

Experimental Data.	Explanation on the Theory of Biedermann.	Explanation on the Theory of Bayliss and Bradford, with Langley's Modification.
The normal "rest current" is ingoing.	Electrical expression of "chemismus" of cells with predominance of katabolism. (Water secretion ?)	Same without statement *re* katabolism.
Cold reverses the ingoing to an outgoing "rest current".	Of the two cell processes the katabolic (ingoing current) is less resistant to abnormal conditions than the anabolic (outgoing current).	No explanation apparent. (Outgoing current is electro-capillary, and demands a flow of fluid.)
Narcotics (CO_2 Ether, Chloroform) and removal of Oxygen reverse ingoing current to outgoing.	The less resistant katabolic process (ingoing current) is more easily affected than the anabolic.	No explanation apparent. (Water passes into cells of glands of *membrana nictitans* under these circumstances, but the swelling soon attains its maximum.)
Abstraction of water reverses the "current of rest" to outgoing.	Diminution of activity of predominant katabolic process.	No explanation apparent. (Outgoing current due to flow of fluid.)
Strong stimulus gives outgoing action current.	Explanation not clear. (Strong stimulus inhibits katabolic process but stimulates anabolic ?)	Electro-capillary current due to flow of fluid in cells swamps the simultaneously excited ingoing current due to increased chemical activity.
Weak stimulus gives ingoing "action current".	More easily excited katabolic process is alone stimulated to action.	Flow of fluid not sufficient to cause a current capable of swamping that due to chemical activity.
Weak ingoing "current of rest" favours ingoing "action current".	The process in abeyance (katabolic) is excited more than the process in prominence (anabolic).	Low "condition"; cells not capable of sufficient response to cause a marked flow of fluid; hence the "physiological current" predominates.
Strong ingoing "current of rest" favours outgoing "action current".	The process in abeyance (anabolic) is excited more than the process in prominence (katabolic).	Good "condition"; cells respond readily, and a strong flow of fluid causes swamping electro-capillary current.

BIBLIOGRAPHY.

DU BOIS-REYMOND. *Moleschott's Untersuchungen*, 1857, und *Unters über thiersche Elektricität*, bd. ii., abth. 2.

BUDGE. *Poggendorff's Annalen*, s. 537, 1860.

VALENTIN. *Zeitschr. f. rat. Med.*, bd. xv., s. 208, 1861.

ROSENTHAL. *Reichert u. Du Bois-Reymond's Archiv*, s. 301, 1865.

ROEBER. *Reichert u. Du Bois-Reymond's Archiv*, s. 633, 1869.

ENGELMANN. *Pflüger's Archiv*, bd. iv., ss. 1 u. 321; bd. v., s. 498; bd. vi., s. 97; bd. xv., ss. 136-138.

HERMANN. *Pflüger's Archiv*, bd. xvii., s. 291, 1878; bd. xxvii., s. 280, 1882.

BACH und OEHLER. *Pflüger's Archiv*, bd. xxii., s. 30, 1880.

BAYLISS and BRADFORD. *Journal of Physiology*, vol. vii., p. 217, 1886.

HERMANN und LUCHSINGER. *Pflüger's Archiv*, bd. xvii., s. 310, 1878; bd. xviii., ss. 460, 878.

LUCHSINGER. *Pflüger's Archiv*, bd. xxii., s. 152, 1880; *Tagebl., d. 52, deutsch Naturforschervers. in Baden Baden*, 1879; *Hermann's Handbuch*, bd. v., th. 1, ss. 441-446, 1883.

TARCHANOFF. *Pflüger's Archiv*, bd. xlvi., 1892.

BAYLISS and BRADFORD. *Proc. Royal Soc.*, vol. xl., p. 203, 1886; *Monatsch f. Anat. u. Physiol.*, bd. iv., s. 1.

BRADFORD. Thesis for D.Sc. degree of University of London. Lewis, London, 1888. *Journal of Physiology*, vol. ix., p. 287.

LANGLEY. *Journal of Physiology*, vol. ix., p. 62, 1888.

BIEDERMANN. *Pflüger's Archiv*, bd. liv., s. 209, 1893.

BOHLEN. *Pflüger's Archiv*, bd. lvii., s. 97, 1894.

WAYMOUTH REID. *Phil. Trans.*, vol. clxxxiv., p. 335, 1893; *Journal of Physiology*, vol. xvi., p. 359, 1894.

WAYMOUTH REID and TOLPUTT. *Journal of Physiology*, vol. xvi., p. 203.

FRITSCH. Die Elektrischen Fische, 1st abth., Malapterurus electricus.

STRICKER und SPINA. *Wien Sitzungsberichte*, bd. lxxx., 3te abth., s. 95, 1880.

DRASCH. *Du Bois-Reymond's Archiv, Jahrg.*, 1889, s. 96.

HERING. *Wien Sitzungsberichte*, bd. lxvi., 3te abth., s. 1, 1872.

LANGLEY. *Journal of Physiology*, vol. x., p. 433.

BIEDERMANN. *Wien Sitzungsberichte*, bd. lxxxvi., 3te abth., s. 67, 1882; bd. xciv., 3te abth., s. 250, 1887.

E. WAYMOUTH REID.

ATOMIC WEIGHTS.

THE study of the quantitative relations between reacting substances was begun long before Dalton announced his atomic theory which did so much to explain them. Centuries before the time of Dalton an atomic theory had been advanced to account for the physical properties of substances from which his theory differed in many ways, although in many points there was a striking similarity. Dalton's theory sprang from the necessity of finding an explanation of facts derived from experiments and it dealt with much smaller particles than the ancient theory. The new theory was a chemical, the old a physical one. Dalton was not contented with a mere statement (1) "that *the ultimate particles of all homogeneous bodies are perfectly alike in weight, figure, etc.* In other words, every particle of water is like every other particle of water; every particle of hydrogen is like every other particle of hydrogen, etc." He went further and attempted to determine the relative weights of these ultimate particles of various elements and compounds. His success in this department of research was not great, and what astonishes us is that with such analytical results he should have enunciated his theory so clearly and comprehensively as he did. Of those who worked in this field of research one name stands out far above all others. Not only for the accuracy of his experimental work, but also for the many ingenious processes and reactions which he employed to arrive at his goal, will the name of Berzelius be ever honoured. Besides these determinations of theoretical interest, the new theory led to an enormous advance in the accuracy of all quantitative analysis, for it is obvious that now one had a standard far more infallible than a mere comparison with the results obtained by other chemists by which one could test the accuracy of analytical processes.

Speculative chemists and physicists tended to go still further back than to the atoms, for, they asked, are not atoms made of something and may not that something be a

simple, or even the simplest form of matter, which forms itself into atoms just as these atoms in turn unite to build up what we call compounds? This idea was much encouraged by a remarkable letter published in 1815 in Thomson's *Annals of Philosophy*. The theory advanced was that the weights of the atoms of all elementary substances were multiples of the weight of an atom of hydrogen. The first letter (2) was followed by a second, both published anonymously, although it was known shortly after that both were by Dr. W. Prout, and the hypothesis received the name of Prout's Theory, or more often, Prout's Law. It was received with great favour by chemists in this country and in France, but by Berzelius and the German chemists it was looked on with suspicion and distrust. Chemists were thus divided into two schools, and to the friendly rivalry between these we are indebted for many pieces of classical research in this department, and for great improvements in the ordinary processes of quantitative analysis. Amongst the first consequences of the increased accuracy of quantitative determinations was the proof that the atomic weight of chlorine could not be regarded any longer as thirty-six times that of hydrogen but only thirty-five and a half, and from this it was evident that the atom of hydrogen as a whole could no longer be taken as the unit for Prout's Theory, and having got down to half an atom it was but a little step further to get down to a quarter of an atom. In this second letter Prout remarks that "we may almost consider the πρώτη ὕλη of the ancients to be realised in hydrogen" but fifteen years later (3) he says that "there seems to be no reason why bodies still lower in the scale than hydrogen (similarly, however, related to one another, as well as to those above hydrogen) may not exist, of which other bodies may be multiples, without being actually multiples of the intermediate hydrogen". It is now generally admitted that Prout's Law must be given up altogether in its original sense, but it must be very clearly borne in mind that this by no means involves the giving up of such views as to the ultimate constitution of matter as those which were held by the chemists who gave the theory a hearty welcome. Nor does

it follow that there is no truth in the law. Far from it; at the end of his paper (4) on the atomic weight of aluminium, Professor Mallet gives a list of the atomic weights of eighteen elements which were then most accurately known, and points out that of these "ten approximate to integers within a range of one-tenth of a unit. What then is the degree of probability that this is purely accidental, as those hold who carry to the extreme the conclusions of BERZELIUS and STAS? . . . The probability in question is found to be only equal to 1 : 1097·8." In the last sentence of the paper Professor Mallet remarks "that not only is Prout's Law not as yet absolutely overturned, but that a heavy and apparently increasing weight of probability in its favour, or in favour of some modification of it, exists and demands consideration".

Another point, indicating how little we know as to the true meaning of striking numerical relationships, is, that when we have three members of the same group of elements the intermediate atomic weight is always *a little less* than the arithmetical mean of the other two. This is clearly shown by the following examples. The atomic weights taken are referred to the standard O = 16.

Lithium	= 7·030	mean 23·083	Magnesium	= 24·38	mean 68·23
Sodium	= 23·058		Zinc	= 65·38	
Potassium	= 39·136		Cadmium	= 112·08	
Sulphur	= 32·06	mean 79·83	Chlorine	= 35·453	mean 81·158
Selenium	= 79·07		Bromine	= 79·963	
Tellurium	= 127·60		Iodine	= 126·864	

Calcium	= 40·00	mean 88·72
Strontium	= 87·52	
Barium	= 137·44	

The consideration of such groups leads us naturally to another point into which we cannot enter here, *viz.*: do the properties of the elements vary in any definite way with their atomic weights? and the answer to this is given by the statement of the Periodic Law as developed by Newlands, Lothar Meyer and Mendelejeff, which states in its simplest form that "*the properties of the elements form a periodic function of the atomic weights of the elements*". This law has received very general acceptance in spite of two or three

obvious exceptions which exist at the present time and which may be relegated to the position required only after very careful and accurate research as to their true atomic weights. With respect to this law as with Prout's, one must not forget that it must express the results of experiments and not override them. To take an example, we must not assign to tellurium an atomic weight less than that of iodine simply because that of oxygen is less than that of fluorine, and that of sulphur less than that of chlorine, nor say that it must be a mixture when all experimental evidence points to the contrary. What we call tellurium *may be* a mixture and true tellurium *may* have an atomic weight less than that of iodine. When this is conclusively proved, *but not till then*, let such an atomic weight be assigned to it.

It is obvious that if we are to have the atomic weights accurately represented by whole numbers this must depend to a very great extent on the unit selected, if it be possible at all. Berzelius adopted oxygen as his standard, making its atomic weight equal to 100. He did this because his values were so largely derived from the oxidation of the elements or the reduction of their oxides so that he obtained his results directly in terms of the atomic weight of oxygen. This standard gives numbers too large for convenient use, and therefore those chemists who believe in referring atomic weights to an oxygen standard have adopted sixteen as its atomic weight, primarily because the atomic weight of oxygen is approximately sixteen times that of hydrogen and for a long time it was supposed to be exactly so. Chemists are still divided in opinion as to what is the most scientific standard for atomic weights. Many, for the same reasons which influenced Berzelius, hold that sixteen for the atomic weight of oxygen is better than one for the atomic weight of hydrogen. To refer very many to the hydrogen standard simply means correcting those on the scale for oxygen by multiplying by a factor, but in not a few cases this cannot be done so simply, especially when we have obtained the number partly by direct reference to hydrogen as well as to oxygen and have taken a mean value. Further, at the present time it can hardly be said to be

definitely settled what is the true ratio between the atomic weights of these two elements. Until this is finally settled it seems absurd to take hydrogen as the standard even though it may be asked: "What is then your unit?" The reply to this is, that the unit is as good, and is of the same kind, as a foot or an inch, which are but subdivisions of the length of the standard yard. A point of some practical importance also is, that if we adopt O = 16 we have a large number of atomic weights which for everyday use may be taken as whole numbers, but this is not so if we take H = 1.

We must next consider the state of our knowledge as to the relative values of the atomic weights of hydrogen and oxygen. The determination of this ratio with an accuracy such as the present state of science demands is one of very great practical difficulty, and in spite of the many attempts and because of the various results obtained by different experimenters we are still undecided.

The methods adopted have been both chemical and physical. The purely chemical methods depend mainly on the determination of—

(1) The composition of water,
(2) ,, ,, ammonia and ammonium salts,
(3) ,, ,, hydrocarbons.

The first of these is of primary importance, and the others mainly of interest in as far as they corroborate (or the reverse) results obtained by it. The physical method is based on the relative densities of the two gases coupled with a knowledge of the proportions by volume in which they combine.

The determination of the composition of water gravimetrically, as is well known, depends mainly on the reduction to the metal of a weighed quantity of copper oxide, which thus gives up a definite quantity of oxygen to form a quantity of water which may be weighed. As Dumas points out in his paper (5) it is a process only calculated to yield good results when carried out with extreme care, because any error in the determination of either

the oxygen or the water is thrown on to the hydrogen, the amount of which is obtained by difference, and this in turn is to be divided into the directly weighed amount of oxygen in order to determine its equivalent. This is theoretically a very serious, if not a fatal defect in the method.

This process was first employed by Dulong and Berzelius, who made three experiments which were not very concordant but yielded (after correction for the volume of air displaced by the water formed, etc.) the value 15·894 for the atomic weight of oxygen. About twenty years afterwards Dumas (5) published first in the *Comptes Rendus* in 1842, and afterwards in the *Annales de Chimie* in 1843, the results of his classical research. In these papers he criticises the results of Dulong and Berzelius, and points out that the weights of copper oxide, copper and water produced, must be corrected to vacuum standard instead of simply being weighed in air. He goes on to give a description of his apparatus and manipulation and gives his weighings from nineteen experiments in tabular form. In these experiments he abstracted from his copper oxide as much as 840·161 grams of oxygen and formed 945·441 grams of water, from which Dumas deduces as the mean of nineteen experiments the ratio of 1 : ·12515 which corresponds to O = 15·98. If we treat the whole series as one huge experiment the value becomes 1 : ·1253 or O = 15·96. As pointed out by Dittmar, Dumas although weighing his copper oxide and metallic copper *in vacuo* seems to have neglected the far more important similar correction for the water produced, even although he points out (as mentioned above) in the same paper that the results of Berzelius required this correction. This seems almost incredible, but in his tabulated results he gives the weight of the water-collecting apparatus before and after each experiment, and takes the simple difference as the true weight of water produced. One or two other sources of error will be referred to later.

The next paper (6) in the same number of the *Annales de Chimie* is by Erdmann and Marchand and is on the same

subject and worked out in a very similar way, although the experiments are not so numerous. The experiments are arranged in two series, in the second of which a notable improvement in the arrangement of the apparatus is made, *viz.*, the introduction of a tube containing metallic copper kept red hot so as to remove every trace of oxygen from the hydrogen, and this is, of course, followed by drying tubes. Four experiments were made in each series, in the first of which the weighings were all reduced to vacuum standard, but in the second series they were actual vacuum weighings. In F. W. Clarke's recalculation of atomic weights (7) the atomic weight of oxygen is given as $15{\cdot}9369 \pm {\cdot}0138$ from the first series and $16{\cdot}0095 \pm {\cdot}0030$ from the second. Clarke's remarks on these results are not quite fair, especially as the experimenters themselves say that they regard the second series as more accurate than the first. He says: "The effect of discussing these two series separately is somewhat startling. It gives to the four experiments in Erdmann and Marchand's second group a weight vastly greater than their other four and Dumas' nineteen taken together. For so great a superiority as this there is no adequate reason; and it is highly probable that it is due almost entirely to fortunate coincidences rather than to greater accuracy of work." If, after making most important improvements in their apparatus so as to obtain purer hydrogen (especially as regards oxygen), as well as modifying their manipulation so as to obtain the true weights of oxygen and of water directly, to say nothing of their experience, better and more concordant results had not been obtained, in what direction could we look for such?

Two other researches carried out in almost exactly the same way as the above are of the highest importance and interest, especially that by Dittmar and Henderson (8), who intended at first only to try to detect the sources of error to which the method was liable, but in the end made a series of experiments which seem to be free from the many sources of error which they proved to exist in the experiments of Dumas. The sources of error which had

escaped notice previously were, firstly, the formation of sulphur dioxide when hydrogen was allowed to stand in contact with strong sulphuric acid, and secondly, as carried out by Erdmann and Marchand, the fact that the escaping gas was only dried by fused calcium chloride, although the gas supplied to the copper oxide was dried by means of phosphoric anhydride and fused potash. Full details are given as to the apparatus used and of each experiment, and the mean of the seven best experiments gives O = 15·866 when H = 1 or H = 1·0085 when O = 16. Leduc (9) has made some similar experiments, but only scanty details are given, and his result is stated as O = 15·882.

A very interesting modification is that of Noyes (10), who used an apparatus entirely of glass, containing copper oxide and having a part which could be cooled so as to condense any water formed. The apparatus with copper oxide was exhausted of air and weighed, then heated and connected with an apparatus supplying pure hydrogen, water was formed and condensed in the cold side tube. After the experiment was finished the apparatus was again exhausted with suitable precautions and the weight of hydrogen which had entered into combination with oxygen was found directly. The whole apparatus was now heated and the water distilled out and the apparatus again exhausted and weighed. We have therefore here directly obtained the weight of hydrogen and that of the oxygen which combined with it, as well as that of the water produced. The value for oxygen so obtained is 15·886 ± ·0028, or, if O = 16, then H = 1·00717.

Cooke and Richards (11) and also Keiser (12), weighed the hydrogen and also the water formed from it and thus escaped the most serious objection to the copper oxide method. The first-named chemists weighed their hydrogen in a large glass globe, which was first weighed empty, weighed again after filling with hydrogen, the hydrogen was then driven first by a current of pure nitrogen and finally by dry air over red hot copper oxide. The water was weighed but not the copper oxide. The great objection to this method is the great size of the glass globe required to

hold any fair quantity of hydrogen, and the difficulty of weighing it accurately. This was done, however, after the manner of Regnault, by having a counterpoise of the same external volume, and the weighings obtained seemed to be satisfactory in the highest degree even under great variations in the atmospheric conditions. Perhaps the most probable source of error is in the assumption that the increase in weight of the globe is due to absolutely pure hydrogen. Oxygen may be completely removed from hydrogen contaminated with it, but this is not so with nitrogen. A trace of nitrogen therefore in the hydrogen would escape detection and defy removal and so cause serious error. An error in Regnault's method of weighing gases was pointed out by Lord Rayleigh after Cooke and Richards' paper was published; this was the correction for the difference in buoyancy of the globe, caused by differences in the external and internal pressures, the globe being naturally smaller when exhausted than when filled with gas at about the atmospheric pressure. Their corrected results give H = 1·00825.

Keiser weighed his hydrogen in the form of palladium hydride, expelling the hydrogen by heat and passing it, mixed with nitrogen, over red hot copper oxide, the water formed being collected and weighed in much the usual way. His results are much higher than those of most of the recent workers in this field, as they point to 15·9492 as the atomic weight of oxygen, if O = 16, H = 1·00318. His paper is too vague in many points for criticism; we are told that "*chemically* pure Zinc, free from arsenic" was used for making the hydrogen, that pure nitrogen was used, but no description of how this was obtained is given, his weights were corrected by the "well-known method of adjustment," and such like statements abound throughout the paper. Keiser seems to have unbounded confidence in the drying power of *one* phosphoric anhydride tube even when passing large quantities of inert gas saturated with aqueous vapour through it at a considerable rate, for we are told that "a combustion including the weighings could be made in four hours," also that the various pieces of the apparatus after

cleaning "were allowed to stand a sufficient time to acquire the temperature of the room and were then weighed" to $\frac{1}{1000}$ of a milligram. One source of error which may have existed and apparently was not investigated sufficiently was the tightness of the tap on the palladium tube, the palladium practically being weighed *in vacuo* before and after the experiment. Air might easily leak in as any one who has had anything to do with the very best glass taps knows, and when it is a three way stopcock the risk is greatly increased. The connection of two of the pieces by ground glass joints wanted careful testing, especially as the joints seem to have been used free from lubricant of any kind. This paper is unsatisfactory, therefore, although of fair length, as it does not give *precise* information where it is most required, but gives, instead of such, only general statements. No arrangement seems to have existed for that most fundamental of all such operations, *viz., testing whether the apparatus was air-tight to begin with.*

Lord Rayleigh (13) used yet another gravimetric method, he abstracted from weighed globes containing hydrogen and oxygen a quantity of each gas and combined them by successive explosions and then determined the small volume of the residual gas and found its weight by calculation. The loss of weight of the respective globes after due correction for the change of volume consequent on the changed internal pressure, and for the small residual volume of one gas, gave the weights of the two gases which entered into combination. The value thus derived is O = 15·89 or H = 1·00692.

The ratio of the respective volumes in which the two gases combine to form water has been determined directly by both E. W. Morley (14) and the author (15), but the results arrived at are not the same, nor does either agree with the result deduced by Leduc (16) from his determinations of the densities of hydrogen and of oxygen and that of the mixture of the gases obtained from the electrolysis of strong caustic potash solution, which he seems to *assume* contains the gases in exactly the proportions in which they

would unite, with not the slightest excess of either. The value for the volumes of hydrogen which unite with one volume of oxygen is, according to Morley, 2·00023, Leduc 2·0037, Scott 2·00245.

Undoubtedly Leduc's result has not the same value as a direct measurement, for a very minute error in the density determination of the gaseous mixture would affect the ratio of the volumes very considerably, and he himself points out that variations in the pressure at which the gas is being evolved very markedly affect the density of the mixture and therefore still more so its composition. His ratio of the volumes is, however, of the same order of magnitude as that of the author, whilst that of Morley shows a very much smaller (only about one-tenth) variation from the exact value of 2 : 1.

The apparatus in which the gas is measured by Morley is practically that of the author as described in his preliminary note (17) to the Royal Society in 1887, but with this important difference that it is also used as the exploding vessel, and hence the products of combustion in the form of water are left and must interfere with the accuracy of the readings, since Morley claims that he can read his mercury levels to $\frac{1}{5000}$ of a millimetre and that he does so to $\frac{1}{2000}$ *in a wide tube.* His apparatus for the preparation and purification of his gas is also very complicated and voluminous. His method of testing the residual gas, after the explosion is over, is not at all satisfactory, as it would give it a minimum value, especially if the residue contained hydrogen or oxygen which had escaped combination in the final explosion, and according to Dixon's experiments there seems to be a small quantity which does miss fire in every such explosion, and this agrees quite with the author's experience, although when the gases are *very* nearly in their exact combining proportions it becomes inappreciable, yet when there is, for example, a fair excess of oxygen then always a small quantity of hydrogen also remains, and the greater the excess of oxygen the larger, as a rule, also is the quantity of hydrogen in the residue. Morley states that his "explosions were always made in the presence of a

large volume of inert gas". This is, of course, far from a condition tending to give results of great accuracy. The apparatus used by the author of this paper possessed the following advantages: (1) The gases were admitted to the measuring vessel direct from the generating vessel without having to be transferred by manipulation at a pneumatic trough; (2) the measuring vessel was the same for both gases, the volume being kept constant by varying the pressure at which the gases were measured; (3) the residue in each experiment was measured and analysed and the unabsorbable residue (considered as impurity) was at any rate *a maximum* as it would contain any gas evolved by the liquid re-agents used (as carbonic oxide from the alkaline pyrogallol); (4) the water formed by the combination of the gases was not admitted into the measuring vessel, and so accurate measurement of the residual gas was rendered possible, and this was done in a narrow tube forming part of the measuring vessel; (5) the gases were obtained by a method of fractional preparation, so that any variation in the ratio of the combining volumes would have been detected and any change of composition or purity in the gases made apparent. Thus it was discovered that oxygen from potassium chlorate always gave a low ratio, and that this was due to traces of chlorine escaping careful washing by caustic potash solution was proved by direct experiment afterwards. It is very probably to this impurity that Morley's lower ratio is due, as he used potassium chlorate as the source of his oxygen. In three of the last series of twelve experiments the volume of hydrogen taken exceeded twice that of the oxygen, yet giving a residue of oxygen, thus proving most conclusively that the ratio notably exceeds 2 : 1. The volumes in these experiments were—

Hydrogen.	Oxygen.	Residue of Oxygen.
6870·0	3432·9	2·1
6848·7	3422·1	2·9
6793·1	3395·8	2·1

The result deduced from between forty and fifty experiments is that 100,000 volumes of oxygen unite with 200,245 volumes of hydrogen to form water; or in other words, equal volumes of these gases contain at ordinary temperatures and pressures not exactly an equal number of molecules but in the ratio of 200,245 of oxygen to 200,000 of hydrogen. Applying this to the density of oxygen found by Lord Rayleigh to be 15·882 we get for the atomic weight of oxygen $\frac{15{\cdot}882}{100{\cdot}1225} = 15{\cdot}862$; Dittmar and Henderson found 15·866, Cooke and Richards found 15·869. Leduc (16) found for the ratio of the densities 15·905, and for that of the volumes 1 : 2·0037, giving, therefore, the atomic weight of oxygen a number a little higher, *viz.*, 15·876.

Professor Cooke (18) in 1889 found the density of hydrogen referred to air ·06958, and comparing this with Regnault's density of oxygen 1·1056, deduced the value 15·891 for the density of oxygen referred to hydrogen, and this gives 15·8715 for the atomic weight of oxygen. It is much to be regretted that Professor Cooke did not at the same time redetermine the density of oxygen by his method.

With regard to the composition of the salts of ammonium and their evidence as to the relative atomic weights of hydrogen and oxygen it was pointed out by Stas at the end of his paper (19) of 1860 that if O = 16, NH_4 = 18·06 and N = 14·041, that is, as he says, we have 4·02 instead of 4·00 for H_4. He continues: "L'ensemble de mes recherches me porte à croire que l'erreur existe plutôt sur le poids atomique de l'hydrogène que sur celui de l'azote.

"Si ce fait, sur lequel j'attire, sous toute réserve, l'attention des chimistes, est exact, ce que je compte rechercher bientôt, en reprenant la synthèse de l'eau par une méthode nouvelle, il en résultera que la base sur laquelle le docteur Prout avait établi sa loi, manque elle-même de fondement."

Stas in his paper of 1876 gives 100 parts of silver as equivalent to 49·5987 parts of ammonium chloride and

90·8296 of the bromide. These give us the following values :—

	Ag.	NH_4Cl.	Cl.	NH_4.	N.	H if O = 16	O if H = 1.
Stas	107·930	53·5319	35·457	18·0749	14·055	1·005	or 15·920
van der Plaats	107·9244	53·5291	35·4565	18·0726	14·0519	1·0052	or 15·917
Thomsen	107·9299	53·5318	35·4494	18·0824	14·0396	1·0107	or 15·830

Similarly we have—

	NH_4Br.	Br.	NH_4.	H if O = 16	O if H = 1.
Stas	98·0324	79·952	18·0804	1·00635	15·899
van der Plaats	98·0273	79·9548	18·0725	1·00515	15·918
Thomsen	98·0323	79·9510	18·0813	1·01042	15·835

The final results are therefore not very concordant, since using Stas' own numbers, O = 15·899 to 15·920, with van der Plaats' (21) recalculated values of Stas' atomic weights we get 15·917 to 15·918, with Thomsen's (22) recalculated values of the same, 15·830 to 15·835.

One conclusion at any rate seems certain, and that is, that if H = 1, then O = 16 is too high, thus corroborating the work previously detailed above. In 1883 Professor Dewar and the author (23) attempted to use the substituted ammonias so as to throw additional light on this subject, but the difficulty of obtaining *absolutely* pure triethylamine, free from traces of methyl and the higher radicals, prevented the results from being as conclusive as one would desire. The value 182·012 was obtained for $(C_2 H_5)_3 NHBr$; subtracting Stas' value for ammonium bromide from this, we get 182·012—98·032 = 83·98 for C_6H_{12}, and taking C = 12·003 we get H_{12} = 11·96 or H = ·996. Recently a similar number has been obtained for hydrogen by Professor Thomsen (24) of Copenhagen, by means of ammonia and hydrochloric acid, but by a totally different method. He passes dry hydrochloric acid gas into a weighed flask containing water coloured with litmus and again weighs the flask, this gives the amount of hydrochloric acid absorbed. Dry ammonia gas is now passed in until the

liquid is almost exactly neutral, when it is again weighed. The amount of acid or ammonia in excess is now titrated by means of dilute alkali or acid standard solutions, when the weights of hydrochloric acid and of ammonia equivalent to one another are obtained. One experiment will make the method plain.

Weight of hydrochloric acid . .	5·0191
Added ,, ,, . .	·0172
Total hydrochloric acid . . .	5·0363
Total ammonia	2·3523

$$\frac{H\ Cl}{N\ H_3} = \frac{5{\cdot}0363}{2{\cdot}3523} = 2{\cdot}1410.$$

The mean of all his experiments is 2·14087 ± 0·00009. This when corrected for air displacement becomes 2·13934 ± 0·00009. If we take the values for N and Cl (O = 16) of

Stas	van der Plaats	Thomsen
Cl = 35·457	35·4565	35·4494
N = 14·044 or 14·055	14·0519	14·0396

then it follows—

H = ·9989 or ·9945	·9959	·9992

The uncertainty as to the exact atomic weight of nitrogen has thus a marked influence on the value deduced for H. The values obtained by his recalculation are maintained by Thomsen to give a truer expression to the ratios found by experiment, as existing between the compounds used by Stas, than the atomic weights of either van der Plaats or of Stas himself. It must be noted, however, that if we adopt Thomsen's value for nitrogen, Stas' value for hydrogen is thrown quite the other way, and instead of being H = ·9992 we saw that it must be between 1·0104 and 1·0107.

One point about Thomsen's method is this: "Are the gases really dry, *i.e.*, *absolutely free* from aqueous vapour?" The mere fact of the same ratio being obtained, whether four grams or twelve grams or twenty grams of hydrochloric acid are used in an experiment, only proves that the *quality* of the gas was the same throughout, but not that it was either pure or dry. Unless we have also in one or

more experiments a direct comparison with silver, preferably by Stas' method, we are hardly entitled to use Stas' numbers for the atomic weights of chlorine and nitrogen.

One point ought to be noticed which strongly corroborates the lower value for oxygen (or the higher value for hydrogen), and that is that in many cases we have the atomic weight determined in units derived from oxygen as well as also directly compared to hydrogen. Take, for instance, Mallet's paper (4) " On the Atomic Weight of Aluminium ". There we have—

Al = 27·075 from ammonia alum
27·046 from aluminium bromide

27·0605 mean value if O = 15·96
27·005 from volumes of hydrogen evolved
26·998 from water produced from the hydrogen evolved

27.0015 mean value if H = 1

hence $15{\cdot}96 \times \frac{27{\cdot}0015}{27{\cdot}0605} = 15{\cdot}925$ may be taken as the atomic weight of oxygen if H = 1.

The estimation of the bromine in a brominated hydrocarbon, or the synthesis of hydrochloric acid by means of silver chloride or the chloride of a metal less volatile, or by some volumetric or other method, would give most valuable results.

At the present time, therefore, it would be premature to say that we have settled beyond doubt the relative atomic weights of hydrogen and oxygen.

BIBLIOGRAPHY.

(1) DALTON, J. A new system of Chemical Philosophy, p. 143. Manchester, 1808.
(2) PROUT, W. Thomson's *Annals of Philosophy*, vol. vi., 321, and vol. vii., 111, 1816 and 1817.
(3) PROUT, W. Letter to Professor Daubeny, Sept. 12, 1831, in Daubeny's *Introduction to the Atomic Theory*. Oxford, 1831.
(4) MALLET, J. W. The Atomic Weight of Aluminium, *Phil. Trans.*, clxxi., 1003-1035, 1880.

(5) DUMAS, J. B. Recherches sur la composition de l'eau. *Ann. de Chimie et Phys.* (3), viii., 198, 1843.

(6) ERDMANN and MARCHAND. Über die atomgewichte des Wasserstoffes und des Calciums. *Jour. Prakt. Chem.*, xxvi., 461, 1843.

(7) CLARKE, F. W. A Recalculation of the Atomic Weights, p. 5. *Smithsonian Miscellaneous Collections.* Washington, 1882.

(8) DITTMAR and HENDERSON. On the gravimetric composition of water. *Proc. Phil. Soc., Glasgow*, Dec., 1890.

(9) LEDUC, A. Sur la composition de l'eau et la loi des volumes de Gay-Lussac. *Comptes Rendus*, cxv., 42, 1892.

(10) NOYES, W. A. On the atomic weight of oxygen. *Amer. Chem. Jour.*, xi., 155-161, 1889.

(11) COOKE, J. P., and RICHARDS, T. W. The relative values of the atomic weights of hydrogen and oxygen. *Proc. Amer. Acad. Arts and Sciences*, xxiii., 149; *Amer. Chem. Jour.*, x., 81, 1888.

(12) KEISER, E. H. On the combustion of weighed quantities of hydrogen and on the atomic weight of oxygen. *Amer. Chem. Jour.*, x., 249-261, 1888.

(13) RAYLEIGH, LORD. On the composition of water. *Proc. Roy. Soc.*, xlv., 425, 1889.

(14) MORLEY, E. W. On the volumetric composition of water. *Amer. Jour. Science* (3), xli., 220 and 276, 1891.

(15) SCOTT, A. On the composition of water by volume. *Phil. Trans.*, clxxxiv., 543-568; *Proc. Roy. Soc.*, liii., 130, 1893.

(16) LEDUC, A. Application de la mesure des densités à la détermination du poids atomique de l'oxygène. *Comptes Rendus*, cxv., 313, 1892.

(17) SCOTT, A. On the composition of water by volume. *Proc. Roy. Soc.*, xlii., 396, 1887.

(18) COOKE, J. P. On a new method of determining gas densities. *Proc. Amer. Acad. Arts and Sciences*, xxiv., 202-233.

(19) STAS, J. S. Recherches sur les rapports réciproques des poids atomiques. *Bull. Acad. Roy. de Belgique* (2), x., 208, 1860.

(20) STAS, J. S. De la détermination du rapport proportionnel entre l'argent, les chlorures, et les bromures. *Mem. Acad. Roy. de Belgique*, xliii., 3, Aug. 5, 1876.

(21) VAN DER PLAATS, J. D. Sur les poids atomiques de Stas. *Comptes Rendus*, cxvi., 1363, 1893; Essai de calcul des poids atomiques de M. Stas. *Ann. de Chimie et Physique* (6), vii., 499, 1886.

(22) THOMSEN, J. Über den wahrscheinlichsten Wert der aus den von Stas durchgeführten Untersuchungen sich ableitenden Atomgewichte. *Zeitschrift für physikalische Chemie*, xiii., 726, 1894.

(23) DEWAR, J., and SCOTT, A. On the molecular weights of the substituted ammonias, No. i. Triethylamine. *Proc. Roy. Soc.*, xxxv., 347, 1887.

(24) THOMSEN, J. Experimentelle Untersuchungen zur Feststellung des Verhältnisses zwischen den Atomgewichten des Sauerstoffs und Wasserstoffs. *Zeitschrift für physikalische Chemie*, xiii., 398, 1894.

ALEXANDER SCOTT.

ENUMERATION OF TITLES OF CHEMICAL PAPERS APPEARING IN JUNE, 1894.[1]

American Chemical Journal.

Vol. xvi. No. 6. (June, 1894.)

Urban, L. C., and *Kremers, E.*, The Menthol Group (pp. 395-404). *Urban, L. C.*, and *Kremers, E.*, Ketones from Pinene Derivatives (pp. 404-406). *Phillips, F. C.*, Researches upon the Phenomena of Oxidation and Chemical Properties of Gases (pp. 406-430). *Walker, C.*, The Condensation Products of Aromatic Hydrazides of Acetacetic Ether. Indol and Pyrazol Derivatives (pp. 430-443). *Gibbs, W.*, and *Reichert, E. T.*, A Systematic Study of the Action of definitely related Chemical Compounds upon Animals (pp. 443-449). *Noyes, W. A.*, and *Ballard, H. H.*, The Nitrites of some Amines (pp. 449-457). *Beeson, J. L.*, A Study of the Constituents of the Nodes and Internodes of the Sugar Cane (pp. 457-464). *Roelofsen, J. A.*, On the Solubility of Cream of Tartar in Alcohol of various Strengths and of various Temperatures (pp. 464-467). *Roelofsen, J. A.*, The Iodine-absorption of some of the rarer Fatty Oils (pp. 467-470).

Journal of the American Chemical Society.

Vol. xvi. No. 6. (June, 1894.)

Frankforter, G. B., The Iodides of Narceine (pp. 361-364). *Robinson, N.*, Some Sources of Error in our Methods of determining Potash in Fertilisers and German Potash Salts (pp. 364-372). *M'Ilhiney, P. C.*, Gantter's Process for determining the Iodine Figure of Fats (pp. 372-374). *Stillman, T. B.*, The Chemical and Physical Examination of Portland Cement (pp. 374-385). *M'Ilhiney, P. C.*, The Quantitative Separation of Rosin Oil from Mineral Oils (pp. 385-388). *Howe, J. L.*, Ruthenium and its Nitrosochlorides (pp. 388-396). *Nichls, W.*, An Improved Mercury Thermometer for High Temperatures (pp. 396-398). *Gladding, T. S.*, On the Estimation of Sulphur in Pyrites (pp. 398-405). *Robinson, F. C.*, A Constant Level Apparatus (p. 405). *Nichls, W.*, A Scale of Hardness for Glass (pp. 406-408). *M'Ilhiney, P. C.*, A Method of determining the Koettstorfer Figure of Dark-coloured Substances (pp. 408-410). *Waldbott, S.*, On the Volatility of Borax (pp. 410-418). *Waldbott, S.*, On the Volatility of Sodium Fluoride (pp. 418-420). *Smith, E. F.*, and *Spencer, H. E.*, Electrolytic Separations (pp. 420-425). *Smith, E. F.*, and *Sargent, G. W.*, The Action of Phosphorus Pentachloride upon Molybdenum Trioxide (pp. 425-427). *Baskerville, C.*, The Separation of Titanium from Iron (p. 427).

[1] It is proposed to continue this list for each month.—ED.]

The Analyst.

Vol. xix. No. 219. (June, 1894.)

Harvey, S., A Simple Apparatus for the Extraction of Gases Dissolved in Water (pp. 121-124). *Allen* and *Moor*, On the Detection of Exhausted Ginger (pp. 124-128). *Allen* and *Moor*, On the Change in the Composition of Butter by Long Keeping (pp. 128-131). *Blades, C. M.*, Cheshire Cheese (pp. 131-133). *Teed, F. L.*, Note on the Analysis of Phosphor Tin (p. 133). *Clayton, E. G.*, Note on Lemon and Orange Peel (p. 134).

Journal of the Chemical Society.

Vol. lxv. No. 379. (June, 1894.)

Brauner, B., Fluoroplumbates and Free Fluorine (pp. 393-402). *Perkin, W. H.*, The Magnetic Rotations of some of the Derivatives of Fatty Acids containing Halogens, also of Acetic and Propionic Acids, Phosgene and Ethylic Carbonate (pp. 402-432). *Holt, W.*, and *Sims, W. E.*, Oxidation of the Alkali Metals (pp. 432-445). *Bailey, G. H.*, Volatilisation of Salts during Evaporation (pp. 445-450). *Richardson, A.*, Action of Light on Oxalic Acid (pp. 450-470). *Pears, A., jun.*, Jute Fibre produced in England. II. (pp. 470-472). *Smith, C.*, The Natural Oxycelluloses (pp. 472-479). *Smith, C.*, Note on the Estimation of Furfural (pp. 479-480). *Kipping, F. S.*, α-Hydrindone and its Derivatives. I. (pp. 480-504). *Titherley, A. W.*, Sodium, Potassium, and Lithium Amides (pp. 504-523). *Divers, E.*, and *Haga, T.*, Oximidosulphonates or Sulphazotates (pp. 523-572). *Perkin, W. H., jun.*, The *cis-* and *trans-*Modifications of 1 : 2-Tetramethylenedicarboxylic Acid, and of 1 : 2-Pentamethylenedicarboxylic Acid (pp. 572-591). *Haworth, E.*, and *Perkin, W. H., iun.*, Hexamethylene Dibromide and its Action on Sodium and on Ethylic Sodio-malonate (pp. 591-602).

Journal of the Society of Chemical Industry.

Vol. xiii. No. 5. (31st May, 1894.)

Vautin, C., The Electrolysis of Fused Salts (pp. 448-455). *Stanger, W. H.*, and *Blount, B.*, The Testing of Hydraulic Cements (pp. 455-463). *Gowland, W.*, A Japanese Pseudo-speise (Shiromé) and its Relation to the Purity of Japanese Copper and the Presence of Arsenic in Japanese Bronze (pp. 463-471). *Brown, J. H.*, Treatment of Cupreous Iron Pyrites as carried on at the Portuguese Mines (pp. 472-473). *Tate, F.*, Methods for the Rectification of Vitriol (p. 473). *Thomson, W.*, India-rubber (pp. 473-476). *Weber, C. O.*, On the Analysis of India-rubber Articles (pp. 476-485). *Dreyfus, C.*, On Resist and Discharge Styles with Aniline Black (pp. 485-487). *Procter, H. R.*, On the Qualitative Determination of Tanning Materials (pp. 487-493).

Procter, H. R., Note on the Estimation of Tanning Matter (pp. 494-496). *Perkin, A. G.*, A Rapid Method of Determining the Shade of Alizarine Pastes (p. 496). *Hummel, J. J.*, and *Procter, H. R.*, Note on an Improvement in Dyeing Leather with Basic Colours (pp. 496-497). *Tatlock, R. R.*, and *Thomson, R. T.*, On the Suitability of Waters for Dietetic Purposes as shown by Chemical Analysis (pp. 497-501).

Philosophical Magazine and Journal of Science.

Vol. xxxviii. No. 230. (July, 1894.)

Sutherland, W., The Attraction of Unlike Molecules. I.: The Diffusion of Gases (pp. 1-19).

Chemical News and Journal of Physical Science.

Vol. lxix. No. 1803. (15th June, 1894.)

Linebarger, C. E., Some Modifications of Beckmann's Ebullioscope Apparatus (p. 279).

Vol. lxix. No. 1804. (22nd June, 1894.)

Leicester, J., The Interaction of Quinones and Metanitraniline and Nitroparatoluidine (p. 291). *Warren, H. N.*, The Physical Properties of the Graphites obtained from various Metals after the separation of the same by means of Dilute Acids (p. 291).

Vol. lxix. No. 1805. (29th June, 1894.)

Kern, S., Railway and Carriage Spring Steel (p. 303). *Thomason, W.*, Analysis of Sample of Ancient Slag (p. 303).

Annales de Chimie et de Physique.

Tome ii. (July, 1894.)

Berthelot, Sur une méthode destinée à étudier les échanges gazeux entre les êtres vivants et l'atmosphère qui les entoure (pp. 289-293). *Berthelot* et *André, G.*, Études sur la formation de l'acide carbonique et l'absorption de l'oxygène par les feuilles détachées des plantes (pp. 293-339). *Berthelot* et *Vieille*, Recherches sur quelques sels de l'acide azothydrique (pp. 339-351). *Houllevigue, L.*, Électrolyse et polarisation des mélanges de sels (pp. 351-384). *Minguin*, Étude de quelques dérivés du camphre cyané et de l'éther camphocarbonique (pp. 384-417).

Bulletin de la Société Chimique de Paris.

Tomes xi.-xii. No. 11. (5th June, 1894.)

Freundler, Sur quelques éthers tartriques à radicaux aromatiques (pp. 470-477). *Freundler*, Sur le pouvoir rotatoire des corps dissous (pp. 477-480).

Thomas-Mamert, R., Sur les dérivés aminofumariques (pp. 480-486). *Guerbet*, Sur quelques campholates métalliques (pp. 486-491). *Guerbet*, Éthers de l'acide campholique (pp. 491-496). *Cazeneuve, P.*, Sur des laques bleues dérivées de la dibromogallanilide et sur quelques réactions bleues des polyphénols en présence des alcalis (pp. 496-500). *Genvresse, P.*, Sur les sulfones aromatiques (pp. 501-514). *Barillot, E.*, Sur une nouvelle réaction de la colchicine (pp. 514-516). *Barthe, L.*, Nouvelle méthode de dosage de l'acide salicylique et des salicylates (pp. 516-522).

Tomes xi.-xii. No. 12. (20th June, 1894.)

Villiers, A., et *Fayolle*, Sur la recherche de l'acide chlorhydrique (pp. 539-541). *Villiers* et *Fayolle*, Sur la recherche de l'acide bromhydrique (pp. 541-544). *Villiers* et *Fayolle*, Sur le dosage de l'iode (pp. 544-546). *Sabatier, P.*, Remarques sur certains chlorures métalliques hydratés (pp. 546-549). *Delépine*, Action du chlorobromure de méthylène sur l'ammoniaque en solution dans l'alcool méthylique (pp. 549-556). *Delépine*, Action du chlorure de méthylène sur l'ammoniaque en solution alcoolique (p. 556). *Barral, E.*, Préparation de l'hexachlorophénol α (pp. 557-560). *Massol*, Etude thermique des acides nitrobenzoïques. Influence de l'isomérie. Influence de la substitution nitrée (pp. 560-562). *Sisley, P.*, Sur la préparation des acides acétylgallique et acétyldibromgallique et le dosage de l'acétyle dans ces combinaisons (pp. 562-568). *Matignon, C.*, Sur l'énergie totale mise en jeu dans l'organisme par la combustion des albuminoïdes (pp. 568-571). *Matignon, C.*, Sur l'hydrate d'acide urique (pp. 571-573). *Matignon, C.*, Dérivés métalliques de la formylurée, de l'acétylurée et de l'acide oxalurique (pp. 573-575). *Matignon, C.*, Sur quelques nouveaux sels d'urée (p. 575). *Reychler, A.*, L'essence d'ylang-ylang (pp. 576-583).

Comptes Rendus hebdomadaires des séances de l'Académie des Sciences.

Tome cxviii. No. 23. (4th June, 1894.)

Rousseau, G., et *Allaire, H.*, Nouvelles recherches sur les boracites chlores (pp. 1255-1258). *Charpy*, Sur le rôle des transformations du fer et du carbone dans le phénomène de la trempe (pp. 1258-1260). *Sabatier, P.*, Sur un bromhydrate de bromure cuivrique et sur un bromure rouge de cuivre et de potassium (pp. 1260-1263). *Engel, R.*, Sur la séparation analytique du chlore et du brome (pp. 1263-1265). *Villiers, A.*, et *Fayolle*, Sur la recherche de l'acide bromhydrique (pp. 1265-1268). *Barthe, L.*, Nouveaux dérivés des éthers cyanacétique et cyanosuccinique (pp. 1268-1271). *Klobb, T.*, Combinaisons de la pyridine avec les permanganates (pp. 1271-1273). *Adam, P.*, Sur les émétiques (pp. 1273-1275). *Cavalier, J.*, Sur l'acide monoéthylphosphorique (pp. 1275-1277). *Trillat, A.*, et *Cambier, R.*, Action du trioxyméthylène sur les alcools en présence du perchlorure de fer et sur les

nouveaux dérivés méthyléiques qui en résultent (pp. 1277-1280). *Brochet, A.*, Mécanisme de l'action du chlore sur l'alcool isobutylique (pp. 1280-1282).

Tome cxviii. No. 24. (11th June, 1894.)

Villiers, A., et *Fayolle*, Sur le dosage de l'iode (pp. 1332-1335). *Hitzel, E.*, Sur les sulfates acides d'aniline, d'ortho, et de paratoluidine (p. 1335). *Combes, A.*, Synthèse de dérivés hexaméthylènique, triéthylphloglucine (pp. 1336-1339). *Béhal, A.*, et *Choay, E.*, Composition qualitative des créosotes officinales de bois de hêtre et de bois de chêne (pp. 1339-1342). *Simon*, Action des bases aromatiques primaires sur les composés cétoniques dissymétriques (pp. 1342-1345). *Burcker, E.*, Sur la stabilité des dissolutions aqueuses de bichlorure de mercure (pp. 1345-1347). *Besson, A.*, Sur la préparation de l'éthylène perchloré et l'action de l'oxygène ozonisé sur ce corps (pp. 1347-1350). *Griffiths, A. B.*, Sur une ptomaïne extraite des urines dans le cancer (p. 1350).

Tome cxviii. No. 25. (18th June, 1894.)

Berthelot, Le principe du travail maximum et l'entropie (pp. 1378-1393). *Villiers, A.*, et *Fayolle*, Recherche des traces de chlore (pp. 1413-1415). *Maumené, E.*, Sur les émétiques (pp. 1415-1418).

Journal de Pharmacie et de Chimie.

Tome xxix. No. 12. (15th June, 1894.)

Petit et *Terrat*, Du chlore dans l'urine (pp. 585-591). *Ehrmann*, Étude du henné (pp. 591-598).

Tome xxx. No. 1. (1st July, 1894.)

Béhal, A., et *Choay, E.*, Sur les points de fusion et d'ébullition de quelques phénols et de leurs éthers benzoïques (pp. 5-8). *Grimbert, L.*, Sur la recherche du bacille typhique dans les eaux (pp. 8-10). *L'Hôte*, Falsification du lait par écrémage (pp. 10-14). *Moreau*, Sur la relation entre le pouvoir rotatoire du camphre et le poids moléculaire de quelques dissolvants (pp. 14-19). *Villiers, A.*, et *Fayolle*, Sur la recherche de l'acide chlorhydrique (pp. 19-24).

Justus Liebig's Annalen der Chemie.

Band cclxxix. Heft 1 und 2. (14th June, 1894.)

Marckwald, W., Ueber die Constitution der Ringsysteme. II. (pp. 1-23). *Wislicenus, W.*, Ueber die Einwirkung von Phenylhydrazin auf Benzalmalonsäureester (pp. 23-27). *Kehrmann, F.*, Ueber raumisomere Chinonoxime. I. Ueber raumisomere Aether und Ester des Monochlorchinonmetaoxims (pp. 27-39). *Eschweiler, W.*, Ueber einige Acetonitrile. II. (pp. 39-45). *Bischoff, C. A.*, und *Walden*,

P., I. Ueber derivative der Glycolsäure (pp. 45-71). II. Ueber derivative der Milchsäure (pp. 71-100). III. Ueber derivative der beiden α-Oxybuttersäuren (pp. 100-118). IV. Ueber derivative der Mandelsäure (pp. 118-130). V. Ueber derivative der Aepfelsäure (pp. 130-138). VI. Ueber derivative der Weinsäure (pp. 138-153). *Bischoff, C.*, VII. Ergebnisse der Studien über die Derivative der ein- und zweibasischen α-Oxysäuren (pp. 153-188). *Knorr, Ludwig*, Untersuchungen in der Pyrazolreihe. I. Ueber die Constitution des Pyrazols (pp. 188-232). II. Ueber die Condensation von Methylhydrazin mit β-Diketonen (pp. 232-237). *Rosengarten, G. D.*, III. Ueber die Condensation von Hydrazin mit Acetylaceton, Acetylacetessigester und Aethylidenacetessigester (pp. 237-244). *Oettinger, B.*, IV. Ueber die Condensation von Hydrazin mit Methylacetylaceton (pp. 244-247). *Knorr, L.*, und *Oettinger, B.*, V. Notiz über die Condensation von Hydrazin mit Dimethylacetylaceton (pp. 247-248). *Sjollema, B.*, VI. Ueber die Condensation von Hydrazin mit Benzylaceton und Benzoylacetessigester (pp. 248-255).

Annalen der Physik und Chemie (*Wiedemann*).

Band lii. Heft 3. (1st June, 1894.)

Elster, J., und *Geitel, H.*, Weitere lichtelectrische Versuche (pp. 433-455). *Heydweiller, A.*, Ueber Villari's kritischen Punkt beim Nickel (pp. 462-473). *Dühring, U.*, Vertheidigung der ursprünglichen Fassung des Gesetzes der correspondirenden Siedetemperaturen gegenüber Hrn. S. Young (pp. 556-589). *Röntgen, W. C.*, Ueber den Einfluss des Druckes auf die Dielectricitätconstanten des Wassers und des Aethylalkohols (pp. 593-604). *Déguisne, C.*, Ueber die Frage nach einer Anomalie des Leitvermögens wässeriger Lösungen bei 4° (pp. 604-607).

Archiv für Pharmacie.

Band ccxxxii. Heft 4. (25th June, 1894.)

Schaer, E., Ueber die alcaloidartigen Reactionen des Acetanilids (pp. 249-261). *Nothnagel, G.*, Ueber Cholin und verwandte Verbindungen, mit besonderer Berücksichtigung des Muscarins (pp. 261-307). *Sieck, W.*, Die schizolysigenen Sekretbehälter, vornehmlich tropischer Heilpflanzen (pp. 307-311). *Flückiger, F. A.*, Australische Manna (pp. 311-315). *Poleck, T.*, Ueber das Jalapin (Scammonin) (pp. 315-321). *Adolphi, W.*, Zur Kenntnis des Espentheeres (pp. 321-328).

Berichte der Deutschen Chemischen Gesellschaft.

Jahrgang xxvii. No. 10. (11th June, 1894.)

Palmer, A. W., Dimethylarsin (p. 1378). *Pickering, S. U.*, Prüfung einiger Eigenschaften von Chlorcalciumlösungen (pp. 1379-1386). *Kahlbaum, G. W. A.*, Selbstthätige, continuirlich wirkende Queck-

silber—Luft-pumpe für chemische Zwecke (pp. 1386-1394). *Niementowski, St.*, Synthesen der Chinolinderivative (pp. 1394-1403). *Weil, H.*, Ueber das Tetramethyldiamidobenzhydrol (pp. 1403-1409). *Ladenburg, A.*, Krystallform des weinsauren β-Pipecolin (p. 1409). *Lange, H.*, Zur Kenntniss der Truxillsäuren (pp. 1410-1416). *Liebermann, C.*, Ueber Truxen (pp. 1416-1418). *Jacobson, V.*, Ueber einige Derivative des Opiazons (pp. 1418-1426). *Nietzki, R.*, und *Schneider, J.*, Ueber einige Derivative des Pseudocumols (pp. 1426-1432). *Wohl, A.*, Ueber die Reduction der Nitroverbindungen (pp. 1432-1438). *Lagodzinski, K.*, Ueber β-Anthrachinon (p. 1438). *Aschan, O.*, Ueber die Campherformel von Bredt (pp. 1439-1446). *Aschan, O.*, Ueber die Bildung des Campherchinons bei der Oxydation der Camphocarbonsäure mit Kaliumpermanganat (pp. 1446-1449). *Noyes, W. A.*, und *Ballard, H. H.*, Ueber die Nitrite einiger Amine (pp. 1449-1452). *Heidenreich, A.*, Ueber Indoxazene (pp. 1452-1456).

Jahrgang xxvii. No. 11. (25th June, 1894.)

Bamberger, Eug., und *Williamson, S.*, Ueber das Dekahydrochinolin (pp. 1458-1478). *Tietze, H.*, Zur Kenntniss des Hexahydrochinolins (pp. 1478-1481). *Lossen, W.*, Eine Reaction der Dihydroxamsäuren (pp. 1481-1482). *Harris, W.*, und *Meyer, Victor*, Ueber den Molecularzustand des Calomeldampfes (pp. 1482-1489). *Chalmot, G. de*, Die natürlichen Oxycellulosen (pp. 1489-1491). *Bischoff, C. A.*, und *Walden, P.*, Ueber Anomalien bei Verkettungen (pp. 1491-1495). *Wallach, O.*, Ueber Oxydationsproducte des Carvons (pp. 1495-1497). *Ciamician, G.*, und *Silber, P.*, Synthese des Benzophloroglucintrimethyläthers (Methylhydrocotoïn oder Benzoylhydrocoton) (pp. 1497-1501). *Koenigs, W.*, Ueber Merochinen und Cincholoipon (pp. 1501-1507). *Traube, W.*, Ueber Isonitramine (pp. 1507-1510). *Pechmann, H. von*, und *Vanino, L.*, Darstellung von Acylsuperoxyden (pp. 1510-1513). *Vahle, Th.*, Ueber einige Derivative des α-Diphenylsemicarbazides und des α-Diphenylsulfosemicarbazides (pp. 1513-1519). *Lachmann, A.*, und *Thiele, J.*, Ueber einige Nitramine der Harnstoffgruppe (pp. 1519-1521). *Vries, H. J. F. de*, Phenylhydrazinformiat, -lactat, Acetyl- und Formyl-phenylhydrazin (pp. 1521-1523). *Einhorn, A.*, und *Willstätter, R.*, Ueber die technische Darstellung des Cocaïns aus seinen Nebenalkaloïden (pp. 1523-1524). *Fischer, E.*, Ueber zwei neue Hexite und die Verbindungen der mehrwerthigen Alkohole mit dem Bittermandelöl (pp. 1524-1538). *Looft, E.*, Ueber das Methylcyclopentenon und einige seiner Derivative; sowie über symmetrisches Cyclopentenon (pp. 1538-1542). *Looft, E.*, Neue Holzölbestandtheile (pp. 1542-1547). *Bamberger, Eug.*, und *Voss, A.*, Ueber Ketotetrahydronaphtalin (pp. 1547-1548). *Bamberger, E.*, Ueber das Phenylhydroxylamin (pp. 1548-1557). *Jaffé, M.*, Ueber das Benzidinsenföl (pp. 1557-1561).

Journal für Praktische Chemie.

Band xlix. No. 12. (6th June, 1894.)

Claus, A., Ueber die Constitution des Benzols (pp. 505-525). *Claus, A.*, und *Reinhard, K.*, Para-Bromchinolin. III. (pp. 525-540). *Vaubel, W.*, Ueber das Verhalten einiger Benzolderivative gegen nascirendes Brom. III. (pp. 540-546). *Donath, E.*, Ueber invertirende Wirkungen des Glycerins (pp. 546-549). *Walter, J.*, Zur Darstellung des Chinolins (pp. 549-551). *Wurgaft, J.*, Condensation von Aldehyden mit α-Naphtohydrochinon und α-Naphtochinon (pp. 551-552).

Zeitschrift für Analytische Chemie.

Jahrgang xxxiii. Heft 3. (June, 1894.)

Schjerning, H., Ueber die quantitative Trennung der in Bierwürze enthaltenen amorphen stickstoffhaltigen organischen Verbindungen (pp. 263-299). *Foerster, F.*, Ueber die Einwirkung von Saüren auf Glas (pp. 299-322). *Foerster, F.*, Ueber die Erscheinungen bei der Verwitterung der Gläser und bei der Zersetzung derselben durch Wasser (pp. 322-335). *Fleitmann, Th.*, Ueber die quantitative Bestimmung der gewöhnlichsten Beimischungen des im Handel vorkommenden Reinnickels oder Walznickels (pp. 335-338). *Vedrödi, V.*, Die Zersetzung der Albuminate durch die Hydrate der Alkalien (pp. 338-341). *Bornträger, H.*, Ueber eine einfache Aufarbeitung der Molybdänrückstände, sowie über einige Erfahrungen bei der Phosphorsaürebestimmung mittelst der Molybdänmethode (pp. 341-343). *Gantter, F.*, Apparat zur Herstellung von 50 mm. Wasserdruck bei der Bestimmung des Kältepunktes der Oele (p. 343). *Gernhardt, V.*, Ueber neue Siedegefässe (p. 344).

Zeitschrift für Anorganische Chemie.

Band vi. Heft 6. (27th June, 1894.)

Lea Carey, Umwandlungen von mechanischer in chemische Energie. III. (pp. 349-355). *Löwenthal, S.*, Zur Kenntnis der chlor- und amidochromsauren Salze (pp. 355-369). *Lea Carey*, Neue Methode zur Bestimmung der relativen Affinitäten einiger Säuren (pp. 369-377). *Arctowski, H.*, Ueber die künstliche Darstellung von Haematit (pp. 377-380). *Smith, E. F.*, und *Wallace, D. L.*, Doppelbromüre von Palladium (pp. 380-384). *Smith, E. F.*, und *Sargent, G. W.*, Ueber die Einwirkung von Phosphorpentachlorid auf Molybdänsäure (pp. 384-386). *Kehrmann, F.*, und *Böhm, E.*, Zur Kenntnis der komplexen anorganischen Säuren. VI. (pp. 386-392). *Arctowski, H.*, Beiträge zur Kenntnis des Lösungsvorganges. II. (pp. 392-410).

Zeitschrift für Physikalische Chemie.

Band xiv. Heft 2. (15th June, 1894.)

Neumann, B., Ueber das Potential des Wasserstoffs und einiger Metalle (pp. 193-231). *Wildermann, M.*, Die Methode von Kohlrausch in ihrer Anwendung zur Untersuchung der elektrischen Leitfähigkeit schwach dissocüerter Stoffe (pp. 231-247). *Wildermann, M.*, Ueber eine weitere Methode zur Bestimmung der elektrischen Leitfähigkeit schwach dissocüerter stoffe (pp. 247-272). *Schlamp, A.*, Zur Dissociationstheorie der Lösungen (pp. 272-286). *Thwing, C. B.*, Eine Beziehung zwischen Dielektrizitätskonstante und chemischer Konstitution des Dielektrikums (pp. 286-301). *Lehmann, O.*, Ueber elektrische Konvektion, Sedimentation und Diffusion (pp. 301-317). *Kohlrausch, F.*, und *Heydweiller, A.*, Ueber reines Wasser (pp. 317-331). *Schuncke, J.*, Ueber die Löslichkeit des Äthyloxydes in Wasser und wässriger Salzsäure (pp. 331-346). *Jones, H. C.*, Ueber die Lösungstension von Metallen (pp. 346-361). *Thorpe, T. E.*, Ueber die Beziehung zwischen der inneren Reibung von Flüssigkeiten und ihrer chemischen Natur (pp. 361-373).

Zeitschrift für Physiologische Chemie.

Band xix. Heft 4 und 5. (23rd June, 1894.)

Klebs, E., Ueber Diamidopropionsäure (pp. 301-339). *Baisch, K.*, Ueber die Natur der Kohlehydrate des normalen Harns. II. (pp. 339-369). *Laves, E.*, Untersuchung des Fettes von Frauenmilch (pp. 369-378). *Schmitz, K.*, Die Eiweissfäulniss im Darm unter dem Einflusse der Milch, des Kefyrs und des Käses (pp. 378-401). *Schmitz, K.*, Die Beziehung der Salzsäure des Magensaftes zur Darmfäulniss (pp. 401-411). *Hoppe-Seyler, F.*, Weitere Versuche über die Diffusion von Gasen in Wasser (pp. 411-422). *Araki, T.*, Ueber die chemischen Aenderungen der Lebensprocesse in Folge von Sauerstoffmangel. IV. (pp. 422-476). *Hoppe-Seyler, F.*, Bemerkungen zur vorstehenden. IV. Mittheilung von Herrn T. Araki über die Wirkungen des Sauerstoffmangels (pp. 476-482). *Schütz, E.*, Ueber das Vorkommen von Fleichmilchsäure in pathologischen Harnen (pp. 482-488). *Bittó, B.*, Ueber die Bestimmung des Lecithingehaltes der Pflanzenbestandtheile (pp. 488-499). *Borissow, P.*, Ueber die giftige Wirkung des Diamids, des Dibenzoyldiamids und über das Vorkommen des Allantoins im Harn (pp. 499-511). *Borissow, P.*, Zur Bestimmung des Cystins im Harn (pp. 511-526).

Atti della Reale Accademia dei Lincei.

Vol. iii. Fascicolo 9. (6th May, 1894.)

Biginelli, Cumarine carbossilate (pp. 451-453). *Angeli*, Azione dell' acido nitroso sopra l' amminocanfora (pp. 453-459).

Vol. iii. Fascicolo 10. (20th May, 1894.)

Alvisi, Ricerche sugli acidi inorganici complessi (pp. 494-499). *Gennari*, Spettrochimica del cumarone e dell' indene (pp. 499-504). *Carrara*, Coefficienti di affinità dei solfuri alchilici per gli joduri alchilici (pp. 504-510). *Angeli*, Sopra un nuovo miscuglio esplosivo (pp. 510-514). *Miolati*, Sulla stabilità delle immidi di acidi bibasici (pp. 515-522).

Gazzetta Chimica Italiana.

Anno xxiv. Fascicolo 5. (31st May, 1894.)

Cossa, A., Sulla reazione di Anderson (pp. 393-397). *Anderlini, F.*, Azione dell' etilenediammina sopra alcuni acidi bicarbossilici (pp. 397-401). *Anderlini, F.*, Azione dell' etilenediammina sulle anidridi di acidi bibasici (pp. 401-407). *Ciamician, G.*, e *Silber, P.*, Sulla costituzione della cotoina (pp. 407-432). *Ampola, G.*, Sopra un composto dell' acido picrico con l'anetol (p. 432). *Magnanini, G.*, e *Bentivoglio, T.*, Azioni dell' anidridi acetica sopra l'acido succinico in presenza di cloruro di calcio (pp. 433-437). *Biginelli, P.*, Metaossiacetofenone e derivati (pp. 437-444). *Tassinari, G.*, Sulle diacidanilidi (pp. 444-450). *Pellizzari, G.*, e *Cuneo, G.*, Ricerche sulla guanidina. IV. Amidoguanidina e suoi derivati alchilici (pp. 450-468). *Gennari, G.*, Spettrochimica del cumarone e dell' indene (pp. 468-474). *Anderlini, F.*, Sulle anidridi suberica, azaleica e sebacica (pp. 474-478). *Montemartini, C.*, Dimorfismo del fluoborato potassico (pp. 478-480).